This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

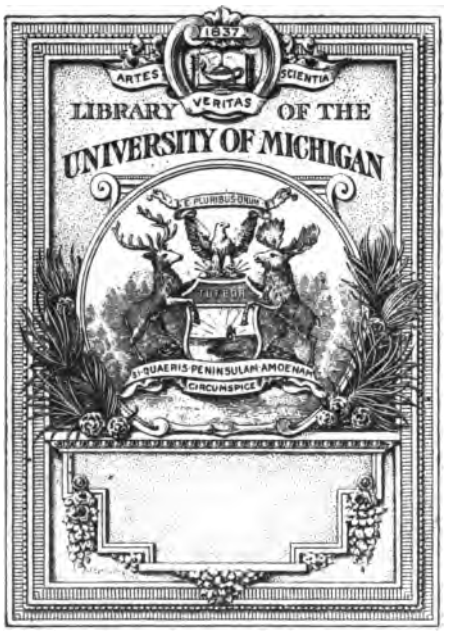
+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

B 1,060,883


1837
ARTES
SCIENTIA
VERITAS
LIBRARY OF THE
UNIVERSITY OF MICHIGAN
E PLURIBUS UNUM
SI QUAERIS PENINSULAM AMOENAM
CIRCUMSPICE

Q
11
.A51p

11.-104

PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

SEVENTEENTH MEETING,

HELD AT

CHICAGO, ILLINOIS,

AUGUST, 1868.

CAMBRIDGE:
PUBLISHED BY JOSEPH LOVERING.
1869.

EDITED BY

JOSEPH LOVERING,

Permanent Secretary.

SALEM:
Essex Institute Press.

TABLE OF CONTENTS.

	PAGE
Officers of the Association,	vi
Special Committees,	viii
Officers of the next Meeting,	ix
Meetings of the Association,	x
Constitution of the Association,	xi
Resolutions and Enactments of a Permanent and Prospective Character,	xvii
List of Members,	xix
Members elected at the Chicago Meeting,	xxxii
EULOGY ON THE LATE A. D. BACHE, by B. A. GOULD,	1
ADDRESS OF THE EX-PRESIDENT, F. A. P. BARNARD,	57

COMMUNICATIONS.

A. MATHEMATICS, PHYSICS, AND CHEMISTRY.

1. On the Application of Electricity to the Maintenance of the Vibrations of the Tuning-fork, and of the Tuning-fork to the Excitement of Vibrations in Cords and Threads. By JOSEPH LOVERING,	103
2. Laws of the Ocean Currents. By J. S. GRIMES,	106
3. The Nature of Electric Discharge. By O. N. STODDARD,	113
4. Influence of the Moon upon the Weather. With Table and Plate. By ELIAS LOOMIS,	118
5. On the Relative Value of Gold and Silver for a series of years. By E. B. ELLIOTT,	122
6. A New Formula for the Reduction of Observations in the Prime Vertical, Analogous to Mayer's Formula for the Reduction of Meridian Observations. By WILLIAM A. ROGERS,	123
7. On the Relation between the Atomic Volume of different metals, and their Paramagnetic and Diamagnetic Properties. By P. H. VANDERWEYDE,	125
8. The Steam Boiler. By J. A. MILLER,	129
9. On the [illegible] of running true or Meridian Lines. By [illegible]	139

10. On the Principles of Statistics as applied to the Census. By FRANKLIN B. HOUGH, ... 145
11. Remarks on the Secular Variations of the Planetary Orbits. With Chart. By J. N. STOCKWELL, ... 159
12. On Hansen's Theory of the Physical Constitution of the Moon. By SIMON NEWCOMB, ... 167
13. The Resuscitation of the Cincinnati Observatory. By CLEVELAND ABBE, 172
14. Phosphoric Acid, a Constituent of Butter. By E. N. HORSFORD, . . 174
15. Fluorine in the Human Brain. By E. N. HORSFORD, ... 176
16. On the Source of Free Hydrochloric Acid in the Gastric Juice. By E. N. HORSFORD, ... 178
17. On the Action of Light upon the Bromide and Iodide of Silver. By M. CAREY LEA, ... 181
18. The Application of Carbonic Acid Gas in the Extinguishment of Fire. By E. L. BUTTRICK, ... 186
19. On the Combining Power of Chemical Elements. By SAMUEL D. TILLMAN, ... 190
20. On Some New Chemical Relations of Metallic Aluminum. By HENRY WURTZ, ... 196
21. On the Condition of our Knowledge of the Processes in Luminous Hydrocarbon Flames. By EUGENE W. HILGARD, ... 201
22. The Meteorites from Poland and Mexico. By LEWIS FEUCHTWANGER, 206
23. The Statics of the Four Types of Modern Chemistry, with especial regard to the Water Type. By GUSTAVUS HINRICHS, ... 209
24. A New and General Law Determining the Atomic Volume and Boiling Point of a Great Number of Carbon Compounds. By GUSTAVUS HINRICHS, ... 223
25. Atomic Motions. By H. F. WALLING, ... 238
26. The Volcanic Tide Belt and the World's Flood Gates. By THEODORE C. HILGARD, ... 252

B. NATURAL HISTORY.

1. On the Evidences of the Antiquity of Man in the United States. By CHARLES WHITTLESEY, ... 268
2. Archæology of the Mississippi Valley. By W. DE HASS, ... 288
3. Report on Archæology and Ethnology. By W. DE HASS, ... 308
4. The New Arctic Continent, or Wrangell's Land, discovered August 14, 1867, by Captain Long, of the American Ship Nile, and seen by Captains Raynor Bliven and others, with a brief notice of Baron Wrangell's Explorations in 1823. By WILLIAM W. WHEILDON, . . . 304
5. The Habitable Features of the North American Continental Plateau, near the line of the Thirty-fifth Parallel of North Latitude. By C. C. PARRY, ... 312
6. On the Leaves of Coniferæ. By THOMAS MEEHAN, ... 317

7. Description of a New Species of Protichnites, from the Potsdam Sandstone of New York. By O. C. MARSH, ... 322
8. On the Preservation of Color in Fossils from Palæozoic Formations. By O. C. MARSH, ... 325
9. Announcement of the Existence of Cretaceous Rocks in Guthrie County, Iowa. By C. A. WHITE, ... 326
10. On the Geology of Lower Louisiana and the Rock Salt Deposit of Petite Anse. By EUGENE W. HILGARD, ... 327
11. Observation on the Red Quartzite Boulders of Western Iowa, and their original Ledges of Red Quartzite in Iowa, Dakota and Minnesota. By C. A. WHITE, ... 340
12. On the Old Lake Beds of the Prairie Region. By SAMUEL JACOB WALLACE, ... 342

PAPERS PRESENTED, BUT ONLY PUBLISHED BY TITLE, ... 345

EXECUTIVE PROCEEDINGS.

History of the Meeting, ... 353
Opening Prayer by Rev. Dr. PATTERSON, ... 353
Welcome, by Hon. J. Y. SCAMMON, ... 354
Reply, by B. A. GOULD, President, ... 355
Officers Elected, ... 359
Next Meeting, ... 359
Communication from BENJAMIN PEIRCE, ... 359
Remarks of F. A. P. BARNARD, on Dr. EHRENBERG, ... 359
Festivities, ... 360
Resolutions Adopted, ... 361
Votes of Thanks, ... 362
List of Railroad and Steamboat Lines granting half fare, ... 362
Report of JOSEPH LOVERING, Permanent Secretary, ... 363
Cash Account of the Permanent Secretary, ... 364–365
Volumes distributed to European Institutions, ... 366

INDEX, ... 369
ERRATA, ... 372

OFFICERS OF THE ASSOCIATION

AT

THE CHICAGO MEETING.

B. A. GOULD, *President.*
CHARLES WHITTLESEY, *Vice-President.*
JOSEPH LOVERING, *Permanent Secretary.*
A. P. ROCKWELL,* *General Secretary.*
A. L. ELWYN, *Treasurer.*

Standing Committee.

EX-OFFICIO.

B. A. GOULD,	J. S. NEWBERRY,
CHARLES WHITTLESEY,	WOLCOTT GIBBS,
JOSEPH LOVERING,	C. S. LYMAN,
A. P. ROCKWELL,*	A. L. ELWYN.

AS CHAIRMEN OF THE SECTIONAL COMMITTEES.

WILLIAM CHAUVENET,	J. D. WHITNEY.

FROM THE ASSOCIATION AT LARGE.

F. A. P. BARNARD,	J. H. COFFIN,
H. L. EUSTIS,	G. W. HOUGH,
H. A. NEWTON,	J. H. RAUCH,
C. C. PARRY,	S. D. TILLMAN.

*As Mr. ROCKWELL was suddenly called home on the first day of the meeting Mr. SIMON NEWCOMB was appointed to fill his place.

Local Committee.

J. YOUNG SCAMMON, *Chairman.*
WILLIAM STIMPSON, *Secretary.*

EDMUND ANDREWS,
J. F. BEATY,
J. V. Z. BLANEY,
E. W. BLATCHFORD,
WILLIAM BROSS,
E. S. CHESBROUGH,
EDWARD DANIELS,
W. E. DOGGETT,
J. W. FOSTER,
DAVID A. GAGE,
WALTER HAY,
E. B. MCCAGG,
JOHN H. RAUCH,
CHARLES H. REED,
J. B. RICE,
T. H. SAFFORD,
E. H. SHELDON,
PERRY H. SMITH,
DANIEL THOMPSON,
GEORGE C. WALKER,
C. L. WILSON,
JOHN M. WOODWORTH.

Local Sub-Committees.

On Reception—Messrs. RAUCH, SAFFORD, MCCAGG, RICE, and BROSS.

On Lodging and Entertainment—Messrs. SCAMMON, MCCAGG, DOGGETT, BROSS, THOMPSON, GAGE and HAY.

On Rooms—Messrs. BLANEY, WALKER and ANDREWS.

On Finance—Messrs. DOGGETT, WALKER, BEATY and BROSS.

On Excursions—Messrs. DANIELS, SHELDON, BEATY, BLATCHFORD, WILSON AND SMITH.

On Invitation, Correspondence and Printing—Messrs. HAY, WOODWORTH and STIMPSON.

On Railroads—Messrs. CHESBROUGH, FOSTER, SHELDON and BLANEY.

SPECIAL COMMITTEES.

A. Committees continued from former Meetings.

1. *Committee to Report in Relation to Uniform Standards in Weights, Measures, and Coinage.*

F. A. P. Barnard,
John F. Frazer,
J. H. Gibbon,
Wolcott Gibbs,
B. A. Gould,
Joseph Henry,
J. E. Hilgard,
John LeConte,
H. A. Newton,
Benjamin Peirce,
W. B. Rogers,
J. L. Smith,
John Torrey.

B. New Committees.

2. *Committee to Audit the Accounts of the Permanent Secretary and the Treasurer.*

B. A. Gould, | H. L. Eustis,

3. *Committee with whom the Permanent Secretary may advise in regard to the publication of the Chicago Proceedings.*

H. A. Newton, | J. S. Newberry.

4. *Committee to act with the Standing Committee in nomination of Officers for the Salem Meeting.*

Section A.	Section B.
Elias Loomis,	E. A. Dalrymple,
Cleveland Abbe,	O. C. Marsh,
J. C. Watson,	C. A. White,
G. W. Hough,	J. W. Foster.

5. *Committee to address Dr. Ehrenberg on the occasion of his approaching Jubilæum.*

Louis Agassiz,
F. A. P. Barnard,
James D. Dana,
B. A. Gould,
Joseph Lovering,
W. S. Sullivant,
John Torrey.

6. *Committee to whom was referred Prof. Benjamin Peirce's communication relating to the changes in the star Eta Argus.*

B. A. Gould, | James C. Watson.
Simon Newcomb.

OFFICERS OF THE SALEM MEETING.

J. W. FOSTER, *President.*
O. N. ROOD, *Vice-President.*
JOSEPH LOVERING, *Permanent Secretary.*
O. C. MARSH, *General Secretary.*
A. L. ELWYN, *Treasurer.*

Standing Committee.

J. W. FOSTER,
O. N. ROOD,
JOSEPH LOVERING,
O. C. MARSH,
B. A. GOULD,
CHARLES WHITTLESEY,
A. P. ROCKWELL,
A. L. ELWYN.

Local Committee.

HENRY WHEATLAND, *Chairman.*
F. W. PUTNAM, *Secretary.*
WILLIAM SUTTON, *Treasurer.*

His Honor, WILLIAM COGGSWELL, *Mayor of Salem.*
C. S. OSGOOD, *President of the Common Council.*

E. S. ATWOOD,
J. H. BATCHELDER,
J. BERTRAM,
E. BICKNELL,
R. BROOKHOUSE,
C. COOKE,
B. COX,
A. CROSBY,
A. W. DODGE,
W. C. ENDICOTT,
A. C. GOODELL, JR.,
L. B. HARRINGTON,
N. A. HORTON,
A. HUNTINGTON,
A. HYATT,
A. H. JOHNSON,
J. KIMBALL,
H. F. KING,
J. C. LEE,
G. B. LORING,
E. S. MORSE,
J. C. OSGOOD,
A. S. PACKARD, JR.,
C. W. PALFRAY,
C. R. PALMER,
S. E. PEABODY,
G. PERKINS,
G. A. PERKINS,
W. P. PHILLIPS,
G. D. PHIPPEN,
C. H. PRICE,
R. S. RANTOUL,
J. ROBINSON,
C. A. ROPES,
G. P. RUSSELL,
B. H. SILSBEE,
A. A. SMITH,
E. SUTTON,
J. C. TOWNE,
W. P. UPHAM,
R. P. WATERS,
B. WEBB, JR.,
B. A. WEST,
S. G. WHEATLAND,
H. L. WILLIAMS,
E. B. WILLSON.

MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Meeting.	Date.	Place.	President.	Vice-President.	General Secretary.	Perman't Secretary.	Treasurer.
1st,	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield,		Walter R. Johnson,		Jeffries Wyman,
2d,	Aug. 14, 1849,	Cambridge, Mass.,	Joseph Henry,		E. N. Horsford,		A. L. Elwyn,
3d,	March 12, 1850,	Charleston, S. C.,	A. D. Bache,*		L. R. Gibbs,*		St. J. Ravenel.*
4th,	Aug. 19, 1850,	New Haven, Ct.,	A. D. Bache,		E. C. Herrick,		A. L. Elwyn.
5th,	May 5, 1851,	Cincinnati, Ohio,	A. D. Bache,		W. B. Rogers,	S. F. Baird,	A. L. Elwyn.
6th,	Aug. 19, 1851,	Albany, N. Y.,	Louis Agassiz,		W. B. Rogers,	S. F. Baird,	A. L. Elwyn.
7th,	July 28, 1853,	Cleveland, Ohio,	Benj. Peirce,		J. D. Dana,	S. F. Baird,	A. L. Elwyn.
8th,	April 26, 1854,	Washington, D. C.,	J. D. Dana,		J. Lawrence Smith,	Joseph Lovering,	J. L. LeConte.*
9th,	Aug. 15, 1855,	Providence, R. I.,	John Torrey,		Wolcott Gibbs,	Joseph Lovering,	A. L. Elwyn.
10th,	Aug. 20, 1856,	Albany, N. Y.,	James Hall,		B. A. Gould,	Joseph Lovering,	A. L. Elwyn.
11th,	Aug. 12, 1857,	Montreal, C. E.,	J. W. Bailey,	Alexis Caswell,	John LeConte,	Joseph Lovering,	A. L. Elwyn.
12th,	April 28, 1858,	Baltimore, M. D.,	Alexis Caswell,*	John E. Holbrook,	W. M. Gillespie,	Joseph Lovering,	A. L. Elwyn.
13th,	Aug. 3, 1859,	Springfield, Mass.,	S. Alexander,	Edward Hitchcock,	William Chauvenet,	Joseph Lovering,	A. L. Elwyn.
14th,	Aug. 1, 1860,	Newport, R. I.,	Isaac Lea,	B. A. Gould,	Joseph LeConte,	Joseph Lovering,	A. L. Elwyn.
15th,	Aug. 15, 1866,	Buffalo, N. Y.,	F. A. P. Barnard,	A. A. Gould,	Elias Loomis,	Joseph Lovering,	A. L. Elwyn.
16th,	Aug. 21, 1867,	Burlington, Vt.,	J. S. Newberry,	Wolcott Gibbs,	C. S. Lyman,	Joseph Lovering,	A. L. Elwyn.
17th,	Aug. 5, 1868,	Chicago, Ill.,	B. A. Gould,	Charles Whittlesey,	Simon Newcomb,*	Joseph Lovering,	A. L. Elwyn.

*In the absence of the regular officer.

CONSTITUTION OF THE ASSOCIATION.*

OBJECTS.

THE Association shall be called THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States, to give a stronger and more general impulse and a more systematic direction to scientific research in our country, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS.

RULE 1. Any person may become a member of the Association upon recommendation in writing by two members, nomination by the Standing Committee, and election by a majority of the members present.

OFFICERS.

RULE 2. The officers of the Association shall be a President, Vice-President, General Secretary, Permanent Secretary, and Treasurer. The President, Vice-President, General Secretary, and Treasurer shall be elected at each meeting for the following one; — the three first named officers not to be reëligible for the next two meetings, and the Treasurer to be reëligible as long as the Association may desire. The Permanent Secretary shall be elected at each second meeting, and also be reëligible as long as the Association may desire.

* Adopted August 25, 1856, and ordered to go into effect at the opening of the Montreal Meeting. Amended at Burlington, August, 1867, and at Chicago, August, 1868.

MEETINGS.

RULE 3. The Association shall meet, at such intervals as it may determine, for one week, or longer,—the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be intrusted to the officers and the Local Committee.

STANDING COMMITTEE.

RULE 4. There shall be a Standing Committee, to consist of the President, Vice-President, Secretaries, and Treasurer of the Association, the officers of the preceding year, the permanent Chairman of the Sectional Committees, after these shall have been organized, and six members present from the Association at large, who shall have attended any of the previous meetings, to be elected upon open nomination by ballot on the first assembling of the Association. A majority of the whole number of votes cast, to elect. The General Secretary shall be Secretary of the Standing Committee.

The duties of the Standing Committee shall be,—

1. To assign papers to the respective sections.
2. To arrange the scientific business of the general meetings, to suggest topics, and arrange the programmes for the evening meetings.
3. To suggest to the Association the place and time of the next meeting.
4. To examine, and, if necessary, to exclude papers.
5. To suggest to the Association subjects for scientific reports and researches.
6. To appoint the Local Committee.
7. To have the general direction of publications.
8. To manage any other general business of the Association during the session, and during the interval between it and the next meeting.
9. In conjunction with four from each Section, to be elected by the Sections for the purpose, to make nominations of officers of the Association for the following meeting.
10. To nominate persons for admission to membership.
11. Before adjourning, to decide which papers, discussions, or other proceedings shall be published.

SECTIONS.

RULE 5. The Association shall be divided into two Sections, and as many sub-sections as may be necessary for the scientific business, the manner of division to be determined by the Standing Committee of the Association. The two Sections may meet as one.

SECTIONAL OFFICERS AND COMMITTEES.

RULE 6. On the first assembling of the Section, the members shall elect upon open nomination a permanent Chairman and Secretary, also three other members, to constitute, with these officers, a Sectional Committee.

The Section shall appoint, from day to day, a Chairman to preside over its meetings.

RULE 7. It shall be the duty of the Sectional Committee of each Section to arrange and direct the proceedings in their Section; to ascertain what communications are offered; to assign the order in which these communications shall appear, and the amount of time which each shall occupy.

The Sectional Committees may likewise recommend subjects for systematic investigation by members willing to undertake the researches, and to present their results at the next meeting.

The Sectional Committee may likewise recommend reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent meetings.

REPORTS OF PROCEEDINGS.

RULE 8. Whenever practicable the proceedings shall be reported by professional reporters, or stenographers, whose reports are to be revised by the Secretaries before they appear in print.

PAPERS AND COMMUNICATIONS.

RULE 9. No paper shall be placed in the programme, unless admitted by the Sectional Committee; nor shall any be read, unless an abstract of it has been previously presented to the Secretary of the Section, who shall furnish to the Chairman the titles of papers, of which abstracts have been received.

RULE 10. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declares such to be his wish before presenting it to the Association.

RULE 11. Copies of all communications, made either to the General Association or to the Sections, must be furnished by the authors; otherwise only the titles, or abstracts, shall appear in the published proceedings.

RULE 12. All papers, either at the general or in the sectional meetings, shall be read, as far as practicable, in the order in which they are entered upon the books of the Association; expect that those which may be entered by a member of the Standing Committee of the Association shall be liable to postponement by the proper Sectional Committee.

RULE 13. If any communication be not ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

RULE 14. No exchanges shall be made between members without authority of the respective Sectional Committees.

GENERAL AND EVENING MEETINGS.

RULE 15. The Standing Committee shall appoint any general meeting which the objects and interests of the Association may call for, and the evenings shall, as a rule, be reserved for general meetings of the Association.

These general meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Sections.

It shall be a part of the business of these general meetings to receive the Address of the President of the last meeting; to hear such reports on scientific subjects as, from their general importance and interests, the Standing Committee shall select; also to receive from the Chairman of the Sections, abstracts of the proceedings of their respective Sections; and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

RULE 16. The Association shall be called to order by the President of the preceding meeting; and this officer having resigned the chair to the President elect, the General Secretary shall then report the number of papers relating to each department which have been registered, and the Association consider the most eligible distribution into Sections, when it shall proceed to the election of the additional members of the Standing Committee in the manner before described; the meeting shall then adjourn, and the Standing Committee, having divided the Association into sections as directed, shall allot to each its place of meeting for the Session. The Sections shall then organize by electing their officers and their representatives in the Nominating Committee, and shall proceed to business.

PERMANENT SECRETARY.

RULE 17. It shall be the duty of the Permanent Secretary to notify members who are in arrears, to provide the necessary stationery and suitable books for the list of members and titles of papers, minutes of the general and sectional meetings, and for other purposes indicated in the rules, and to execute such other duties as may be directed by the Standing Committee or by the Association.

The Permanent Secretary shall make a report annually to the Standing Committee, at its first meeting, to be laid before the Association, of the business of which he has had charge since its last meeting.

All members are particularly desired to forward to the Permanent Secretary, so as to be received before the day appointed for the Association to convene, complete titles of all the papers which they expect to present during its meeting, with an estimate of the time required for reading each, and such abstracts of their contents as may give a general idea of their nature.

Whenever the Permanent Secretary notices any error of fact or unnecessary repetition, or any other important defect in the papers communicated for publication in the proceedings of the Association, he is authorized to commit the same to the author, or to the proper subcommittee of the Standing Committee for correction.

LOCAL COMMITTEE.

RULE 18. The Local Committee shall be appointed from among members residing at, or near, the place of meeting for the ensuing year; and it shall be the duty of the Local Committee, assisted by the officers, to make arrangements and the necessary announcements for the meeting.

The Secretary of the Local Committee shall issue a circular in regard to the time and place of meetings, and other particulars, at least one month before each meeting.

SUBSCRIPTIONS.

RULE 19. The amount of the subscription, at each meeting, of each member of the Association, shall be two dollars, and one dollar in addition shall entitle him to a copy of the proceedings of the annual meeting. These subscriptions shall be received by the Permanent Secretary, who shall pay them over, after the meeting, to the Treasurer.

The admission fee of new members shall be five dollars, in addition to the annual subscription: and no person shall be considered a member of the Association until this admission fee and the subscription for the meeting at which he is elected have been paid.

RULE 20. The names of all persons two years in arrears for annual dues shall be erased from the list of members; provided that two notices of indebtedness, at an interval of at least three months, shall have been previously given.

ACCOUNTS.

RULE 21. The accounts of the Association shall be audited, annually, by auditors appointed at each meeting.

ALTERATIONS OF THE CONSTITUTION.

RULE 22. No article of this constitution shall be altered, or amended, or set aside, without the concurrence of three-fourths of the members present, and unless notice of the proposed change shall have been given at the preceding annual meeting.

RESOLUTIONS.

OF A PERMANENT AND PROSPECTIVE CHARACTER, ADOPTED AUGUST 19, 1857.

1. No appointment may be made in behalf of the Association, and no invitation given or accepted, except by vote of the Association or its Standing Committee.

2. The General Secretary shall transmit to the Permanent Secretary for the files, within two weeks after the adjournment of every meeting, a record of the proceedings of the Association and the votes of the Standing Committee. He shall also, daily, during the meetings, provide the Chairman of the two Sectional Committees with lists of the papers assigned to their Sections by the Standing Committee.

3. All printing for the Association shall be superintended by the Permanent Secretary, who is authorized to employ a clerk for that especial purpose.

4. The Permanent Secretary is authorized to put the proceedings of the meetings to press one month after the adjournment of the Association. Papers which have not been received at that time may be published only by title. No notice of articles not approved shall be taken in the published proceedings.

5. The Permanent Chairmen of the Sections are to be considered their organs of communication with the Standing Committee.

6. It shall be the duty of the Secretaries of the two Sections to receive copies of the papers read in their Sections, all subsections included, and to furnish them to the Permanent Secretary at the close of the meeting.

7. The Sectional Committees shall meet not later than 9 A.M. daily, during the meetings of the Association, to arrange

the programmes of their respective sections, including all subsections, for the following day. No paper shall be placed upon these programmes which shall not have been assigned to the Section by the Standing Committee. The programmes are to be furnished to the Permanent Secretary not later than 11 A.M.

8. During the meetings of the Association, the Standing Committee shall meet daily, Sundays excepted, at 9 A.M., and the Sections be called to order at 10 A.M., unless otherwise ordered. The Standing Committee shall also meet on the evening preceding the first assembling of the Association at each annual meeting, to arrange for the business of the first day; and on this occasion three shall form a quorum.

9. Associate members may be admitted for one, two, or three years as they shall choose at the time of admission,—to be elected in the same way as permanent members, and to pay the same dues. They shall have all the social and scientific privileges of members, without taking part in the business.

10. No member may take part in the organization and business arrangement of both the Sections.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

A.

Abbe, Cleveland, Cincinnati, Ohio (16).
*Adams, C. B., Amherst, Massachusetts (1).
Adelberg, Justus, New York, New York (15).
Aiken, W. E. A., Baltimore, Maryland (12).
Ainsworth, J. G., Barry, Massachusetts (14).
Albert, Augustus J., Baltimore, Maryland (12).
Alexander, Stephen, Princeton, New Jersey (1).
Allen, Zachariah, Providence, Rhode Island (1).
*Ames, M. P., Springfield, Massachusetts (1).
Andrews, E. B., Marietta, Ohio (7).
Angell, James B., Burlington, Vermont (16).
*Appleton, Nathan, Boston, Massachusetts (1).

B.

*Bache, Alexander D., Washington, District of Columbia (1).
Bacon, John, Jr., Boston, Massachusetts (1).
*Bailey, J. W., West Point, New York (1).
Baird, S. F., Washington, District of Columbia (1).
Bardwell, F. W., Jacksonville, Florida (13).
Barker, G. F., New Haven, Connecticut (13).
Barlow, Thomas, Canastota, New York (7).
Barnard, F. A. P., New York, New York (7).
Barnard, Henry, Madison, Wisconsin (12).
Barnard, J. G., Washington, District of Columbia (14).
Barnes, James, Springfield, Massachusetts (5).

NOTE.—Names of deceased members are marked with an asterisk [*]. The figure at the end of each name refers to the meeting at which the election took place.

Barratt, Joseph, Middletown, Connecticut (13).
Basnett, Thomas, Ottawa, Illinois (8).
Batchelder, J. M., Cambridge, Massachusetts (8).
Beadle, E. R., Hartford, Connnecticut (10).
* Beck, C. F., Philadelphia, Pennsylvania (1).
* Beck, Lewis C., New Brunswick, New Jersey (1).
* Beck, T. Romeyn, Albany, New York (1).
Bell, Samuel N., Manchester, New Hampshire (7).
Benedict, G. W., Burlington, Vermont (16).
Bigelow, George H., Burlington, Vermont (16).
* Binney, Amos, Boston, Massachusetts (1).
* Binney, John, Boston, Massachusetts (3).
Bird, William A., Buffalo, New York (15).
Blake, Eli W., Ithaca, New York (15).
Blake, Eli W., New Haven, Connecticut (1).
Blake, W. P., San Francisco, California (2).
* Blanding, William, Rhode Island (1).
Blaney, J. Van Zandt, Chicago, Illinois (12).
* Bomford, George, Washington, District of Columbia (1).
Bouvé, Thomas T., Boston, Massachusetts (1).
Bowditch, Henry I., Boston, Massachusetts (2).
Bradford, Isaac, Jacksonville, Florida (14).
Bradish, Alvah, Fredonia, New York (15).
Bradley, L., Jersey City, New Jersey (15).
Brayley, James, Buffalo, New York (15).
Brevoort, J. Carson, Brooklyn, New York (1).
Briggs, A. D., Springfield, Massachusetts (13).
Bross, William, Chicago, Illinois (7).
Brown, Robert, Jr., Cincinnati, Ohio (11).
Brush, George J., New Haven, Connecticut (11).
Buchanan, Robert, Cincinnati, Ohio (2).
Buck, C. E., New York, New York (15).
* Burnap, G. W., Baltimore, Maryland (12).
* Burnett, Waldo I., Boston, Massachusetts (1).
Bushee, James, Worcester, Massachusetts (9).
Butler, Thomas B., Norwalk, Connecticut (10).

C.

* Carpenter, Thornton, Camden, South Carolina (7).
* Carpenter, William M., New Orleans, Louisiana (1).
Case, Leonard, Cleveland, Ohio (15).
* Case, William, Cleveland, Ohio (6).
Cassels, J. L., Cleveland, Ohio (7).
Caswell, Alexis, Providence, Rhode Island (2).
Cattell, William C., Easton, Pennsylvania (15).
Chadbourn, P. A., Madison, Wisconsin (10).

Chapin, A. L., Beloit, Wisconsin (14).
* Chapman, N., Philadelphia, Pennsylvania (1).
Chase, George I., Providence, Rhode Island (1).
* Chase, S., Dartmouth, New Hampshire (2).
Chauvenet, William, St. Louis, Missouri (1).
Chesbrough, E. S., Chicago, Illinois (2).
Chester, Albert H., New York, New York (15).
Chester, Albert T., Buffalo, New York (15).
Chittenden, L. E., New York, New York (14).
Churchill, Marlborough, Sing Sing, New York (13).
Clapp, Almon M., Buffalo, New York (15).
* Clapp, Asahel, New Albany, Indiana (1).
* Clark, Joseph, Cincinnati, Ohio (5).
Clark, Henry, Worcester, Massachusetts (14).
Cleaveland, C. H., Cincinnati, Ohio (9).
* Cleveland, A. B., Cambridge, Massachusetts (2).
Clinton, George W., Buffalo, New York (15).
Clum, Henry A., New York, New York (9).
Coakley, George W., New York, New York (5).
Cochran, D. H., Brooklyn, New York (15).
Coffin, C. C., Malden, Massachusetts (13).
Coffin, James H., Easton, Pennsylvania (1).
Coffin, John H. C., Washington, District of Columbia (1).
* Cole, Thomas, Salem, Massachusetts (1).
* Coleman, Henry, Boston, Massachusetts (1).
Comstock, C. B., West Point, New York (14).
Conant, Marshall, Washington, District of Columbia (7).
Conkling, Frederick A., New York, New York (11).
Conway, M. D., Cincinnati, Ohio (14).
Copes, Joseph S., New Orleans, Louisiana (11).
Corning, Erastus, Albany, New York (6).
Craig, B. F., Washington, District of Columbia (15).
Cramp, J. M., Acadia College, Nova Scotia (11).
Credner, Herman, New York, New York (15).
Crosby, Alpheus, Salem, Massachusetts (10).
Cummings, Joseph, Middletown, Connecticut (13).

D.

Dalrymple, E. A., Baltimore, Maryland (11).
Dana, James D., New Haven, Connecticut (1).
Danforth, Edward, Troy, New York (11).
Davis, James, Boston, Massachusetts (1).
Dawson, J. W., Montreal, Canada (10).
Dean, Amos, Albany, New York (6).
Dean, George W., Fall River, Massachusetts (15).
* Dearborn, George H. A. S., Roxbury, Massachusetts (1).

Barratt, Joseph, Middletown, Connecticut (13).
Basnett, Thomas, Ottawa, Illinois (8).
Batchelder, J. M., Cambridge, Massachusetts (8).
Beadle, E. R., Hartford, Connnecticut (10).
* Beck, C. F., Philadelphia, Pennsylvania (1).
* Beck, Lewis C., New Brunswick, New Jersey (1).
* Beck, T. Romeyn, Albany, New York (1).
Bell, Samuel N., Manchester, New Hampshire (7).
Benedict, G. W., Burlington, Vermont (16).
Bigelow, George H., Burlington, Vermont (16).
* Binney, Amos, Boston, Massachusetts (1).
* Binney, John, Boston, Massachusetts (3).
Bird, William A., Buffalo, New York (15).
Blake, Eli W., Ithaca, New York (15).
Blake, Eli W., New Haven, Connecticut (1).
Blake, W. P., San Francisco, California (2).
* Blanding, William, Rhode Island (1).
Blaney, J. Van Zandt, Chicago, Illinois (12).
* Bomford, George, Washington, District of Columbia (1).
Bouvé, Thomas T., Boston, Massachusetts (1).
Bowditch, Henry I., Boston, Massachusetts (2).
Bradford, Isaac, Jacksonville, Florida (14).
Bradish, Alvah, Fredonia, New York (15).
Bradley, L., Jersey City, New Jersey (15).
Brayley, James, Buffalo, New York (15).
Brevoort, J. Carson, Brooklyn, New York (1).
Briggs, A. D., Springfield, Massachusetts (13).
Bross, William, Chicago, Illinois (7).
Brown, Robert, Jr., Cincinnati, Ohio (11).
Brush, George J., New Haven, Connecticut (11).
Buchanan, Robert, Cincinnati, Ohio (2).
Buck, C. E., New York, New York (15).
* Burnap, G. W., Baltimore, Maryland (12).
* Burnett, Waldo I., Boston, Massachusetts (1).
Bushee, James, Worcester, Massachusetts (9).
Butler, Thomas B., Norwalk, Connecticut (10).

C.

* Carpenter, Thornton, Camden, South Carolina (7).
* Carpenter, William M., New Orleans, Louisiana (1).
Case, Leonard, Cleveland, Ohio (15).
* Case, William, Cleveland, Ohio (6).
Cassels, J. L., Cleveland, Ohio (7).
Caswell, Alexis, Providence, Rhode Island (2).
Cattell, William C., Easton, Pennsylvania (15).
Chadbourn, P. A., Madison, Wisconsin (10).

Fitch, Edward H., Ashtabula, Ohio (11).
Fitch, O. H., Ashtabula, Ohio (7).
*Forbush, E. B., Buffalo, New York (15).
Force, Peter, Washington, District of Columbia (4).
Fosgate, Blanchard, Auburn, New York (7).
Foster, J. W., Chicago, Illinois (1).
*Fox, Charles, Grosse Isle, Michigan (7).
Frothingham, Frederick, Buffalo, New York (11).

G.

Gay, C. C. F., Buffalo, New York (15).
*Gay, Martin, Boston, Massachusetts (1).
Gibbes, L. R., Charleston, South Carolina (1).
Gibbon, J. H., Charlotte, North Carolina (3).
Gibbs, Walcott, Cambridge, Massachusetts (1).
*Gillespie, W. M., Schenectady, New York (10).
Gillmore, Q. A., Jr., New York, New York (13).
Gilman, Daniel C., New Haven, Connecticut (10).
*Gilmor, Robert, Baltimore, Maryland (1).
Glynn, James, Geneva, New York (1).
Gold, Theodore S., West Cornwall, Connecticut (4).
Goodwin, William F., Richmond, Virginia (10).
*Gould, Augustus A., Boston, Massachusetts (11).
*Gould, B. A., Boston, Massachusetts (2).
Gould, B. A., Cambridge, Massachusetts (2).
*Graham, James D., Washington, District of Columbia (1).
Gray, Asa, Cambridge, Massachusetts (1).
*Gray, James H., Springfield, Massachusetts (6).
Green, Traill, Easton, Pennsylvania (1).
*Greene, Benjamin D., Boston, Massachusetts (1).
Greene, Samuel, Woonsocket, Rhode Island (9).
*Griffith, Robert E., Philadelphia, Pennsylvania (1).
Grinnan, A. G., Orange Court House, Virginia (7).
Grote, Augustus R., Buffalo, New York (15).
Guyot, Arnold, Princeton, New Jersey (1).

H.

*Hackley, Charles W., New York, New York (4).
Hadley, George, Buffalo, New York (6).
Haldeman, S. S., Columbia, Pennsylvania (1).
*Hale, Enoch, Boston, Massachusetts (1).
Hall, James, Albany, New York (1).
Hall, N. K., Buffalo, New York (7).
Hamlin, A. C., Bangor, Maine (10).
Hammond, George T., Newport, Rhode Island (14).
Hance, Ebenezer, Morrisville, Pennsylvania (7).

Hand, Thomas J., Baltimore, Maryland (12).
Hanover, M. D., Cincinnati, Ohio (13).
*Hare, Robert, Philadelphia, Pennsylvania (11).
*Harlan, Joseph G., Haverford, Pennsylvania (8).
*Harlan, Richard, Philadelphia, Pennsylvania (1).
Harman, Henry M., Baltimore, Maryland (12).
*Harris, Thaddeus W., Cambridge, Massachusetts (1).
Harrison, B. F., Wallingford, Connecticut (11).
*Hart, Simeon, Farmington, Connecticut (1).
Hartshorne, Henry, Philadelphia, Pennsylvania (12).
Harvey, Charles W., Buffalo, New York (15).
Harvey, Leon F., Buffalo, New York (15).
Haven, Samuel F., Worcester, Massachusetts (9).
*Hayden, H. H., Baltimore, Maryland (1).
Hayes, George E., Buffalo, New York (15).
*Hayward, James, Boston, Massachusetts (1).
Henry, Joseph, Washington, District of Columbia (1).
Herzer, W., Columbus, Ohio (15).
Hickok, M. J., Scranton, Pennsylvania (11).
Hickok, W. C., Burlington, Vermont (16).
Hilgard, Eugene W., Oxford, Mississippi (11).
Hilgard, Julius E., Washington, District of Columbia (4).
Hill, S. W., Hancock, Lake Superior (6).
Hill, Thomas, Waltham, Massachusetts (3).
Hitchcock, Charles H., Hanover, New Hampshire (11).
*Hitchcock, Edward, Amherst, Massachusetts (1).
Hitchcock, Edward, Amherst, Massachusetts (4).
Hoadley, E. S., Springfield, Massachusetts (13).
Homes, Henry A., Albany, New York (11).
Horsford, E. N., Cambridge, Massachusetts (1).
*Horton, William, Craigville, Orange Co., New York (1).
Hough, Franklin B., Lowville, New York (4).
Hough, G. W., Albany, New York (15).
*Houghton, Douglas, Detroit, Michigan (1).
*Howland, Theodore, Buffalo, New York (15).
Howell, Robert, Nichols, New York (6).
Hubbard, Oliver P., Hanover, New Hampshire (1).
*Hubbert, James, Richmond, Province of Quebec (16).
Hungerford, Edward, Burlington, Vermont (10).
*Hunt, Freeman, New York, New York (11).
Hunt, George, Providence, Rhode Island (9).
*Hunt, E. B., Washington, District of Columbia (2).
Hunt, Sterry, Montreal, Canada (1).
Hyatt, James, Bangall, New York (10).

I.

*Ives, Thomas P., Providence, Rhode Island (10).

J.

Jackson, Charles T., Boston, Massachusetts (1).
Jillson, B. C., Nashville, Tennessee (14).
*Johnson, W. R., Washington, District of Columbia (1).
Johnston, John, Middletown, Connecticut (1).
*Jones, Catesby A. R., Washington, District of Columbia (8).
Joy, C. A., New York, New York (8).
Judd, Orange, New York, New York (4).

K.

Keely, G. W., Waterville, Maine (1).
Keep, N. C., Boston, Massachusetts (13).
Kerr, W. C., Raleigh, North Carolina (10).
Kimball, J. P., New York, New York (15).
King, Mary B. A., Rochester, New York (15).
Kirkpatrick, James A., Philadelphia, Pennsylvania (7).
Kirkwood, Daniel, Canonsburg, Pennsylvania (7).
Kite, Thomas, Cincinnati, Ohio (5).

L.

Lapham, Increase A., Milwaukee, Wisconsin (3).
*Lasel, Edward, Williamstown, Massachusetts (1).
Lattimore, S. A., Lima, New York (15).
Lauderdale, John V., Geneseo, New York (10).
Lawrence, George N., New York, New York (7).
Lea, Isaac, Philadelphia, Pennsylvania (1).
LeConte, John, Columbia, South Carolina (3).
LeConte, John L., Philadelphia, Pennsylvania (1).
LeConte, Joseph, Columbia, South Carolina (3).
*Lederer, Baron von, Washington, District of Columbia (1).
Lee, John R., Buffalo, New York (15).
Leonard, A. M., Lockport, New York (15).
Lesley, Joseph, Jr., Philadelphia, Pennsylvania (8).
Lesley, J. P., Philadelphia, Pennsylvania (2).
Letchworth, William P., Portage, New York (15).
*Lieber, Oscar M., Columbia, South Carolina (8).
*Lincklaen, Ledyard, Cazenovia, New York (1).
Lindsley, J. B., Nashville, Tennessee (1).
*Linsley, James H., Stafford, Connecticut (1).
Little, George, Oxford, Mississippi (15).
Litton, A., St. Louis, Missouri (12).
Locke, Joseph M., Cincinnati, Ohio (13).
Locke, Luther F., Nashua, New Hampshire (7).
Logan, William E., Montreal, Canada (1).
Loomis, Elias, New Haven, Connecticut (1).
Loomis, L. C., Wheeling, West Virginia (9).

Loomis, Silas L., Washington, District of Columbia (7).
Loosey, Charles F., New York, New York (12).
Lord, John, Stamford, Connecticut (13).
Lothrop, Joshua R., Buffalo, New York (15).
Lovering, Joseph, Cambridge, Massachusetts (2).
Lunn, William, Montreal, Canada (11).
Lyman, B. S., Philadelphia, Pennsylvania (15).
Lyman, Chester S., New Haven, Connecticut (4).
Lynch, P. N., Charleston, South Carolina (2).

M.

Mahan, D. H., West Point, New York (9).
Marcy, Oliver, Evanston, Illinois (10).
Marissal, F. V., Fall River, Massachusetts (14).
* Marsh, Dexter, Greenfield, Massachusetts (1).
Marsh, O. C., New Haven, Connecticut (15).
Marshall, Charles D., Buffalo, New York (15).
Marshall, Orasmus H., Buffalo, New York (15).
* Mather, William W., Columbus, Ohio (1).
Mauran, J., Providence, Rhode Island (2).
Mayhew, D. P., Ypsilanti, Michigan (13).
Maynard, Alleyne, Cleveland, Ohio (7).
M'Conihe, Isaac, Troy, New York (4).
McRae, John, Camden, South Carolina (3).
Meade, George G., Philadelphia, Pennsylvania (15).
Means, A., Oxford, Georgia (5).
Meek, F. B., Washington, District of Columbia (6).
Meigs, James A., Philadelphia, Pennsylvania (12).
Miles, Henry H., Quebec, Canada East (11).
Miller, Samuel, New Haven, Connecticut (14).
Minifie, William, Baltimore, Maryland (12).
Mitchell, Maria, Poughkeepsie, New York (4).
Morgan, DeWitt C., Baltimore, Maryland (14).
Morgan, Lewis H., Rochester, New York (10).
Morris, D., Ellingham, Connecticut (14).
Morris, John G., Baltimore, Maryland (12).
Morris, J. R., Houston, Texas (11).
* Morton, S. G., Philadelphia, Pennsylvania (1).
Munroe, Nathan, Bradford, Massachusetts (6).
Murray, David, New Brunswick, New Jersey (11).

N.

Nason, Henry B., Beloit, Wisconsin (13).
Nelson, Cleland K., Annapolis, Maryland (12).
Newberry, J. S., New York, New York (5).
Newcomb, Simon, Washington, District of Columbia (13).

* Newton, E. H., Cambridge, New York (1).
Newton, Hubert A., New Haven, Connecticut (6).
Newton, Robert S., New York, New York (15).
* Nicollett, J. N., Washington, District of Columbia (1).
Niles, W. H., Cambridge, Massachusetts (16).
* Norton, J. P., New Haven, Connecticut (1).
Norton, W. A., New Haven, Connecticut (6).
Nye, A. H., Buffalo, New York (15).

O.

* Oakes, William, Ipswich, Massachusetts (1).
Oliver, James Edward, New York, New York (7).
* Olmsted, Alexander F., New Haven, Connecticut (4).
* Olmsted, Denison, New Haven, Connecticut (1).
* Olmsted, Denison, Jr., New Haven, Connecticut (1).
Ordway, John M., Providence, Rhode Island (9).

P.

Packard, A. S., Jr., Salem, Massachusetts (16).
Paine, Cyrus F., Rochester, New York (12).
Painter, Minshall, Lima, Pennsylvania (7).
Palmer, Everard, Buffalo, New York (15).
Parker, Henry E., Hanover, New Hampshire (11).
* Parkman, Samuel, Boston, Massachusetts (1).
Parmelee, Dubois D., New York, New York (15).
Parry, Charles C., Washington, District of Columbia (6).
Parvin, Theodore S., Iowa City, Iowa (7).
Pease, Francis S., Buffalo, New York (15).
Peck, Edward W., Burlington, Vermont (16).
Peck, William G., New York, New York (13).
Peirce, Benjamin, Cambridge, Massachusetts (1).
Peirce, James M., Cambridge, Massachusetts (11).
Perkins, George R., Utica, New York (1).
Perkins, Maurice, Schenectady, New York (15).
Perry, John B., Brookline, Massachusetts (16).
* Perry, M. C., New York, New York (10).
Phelps, Almira L., Baltimore, Maryland (13).
Phelps, Charles E., Baltimore, Maryland (13).
Pierce, Henry M., New York, New York (15).
Pierrepont, H. E., Brooklyn, New York (14).
Plant, I. C., Macon, Georgia (3).
* Plumb, Ovid, Salisbury, Connecticut (9).
Poole, Henry W., Boston, Massachusetts (14).
Pope, Charles A., St. Louis, Missouri (12).
* Porter, John A., New Haven, Connecticut (14).
Powell, Samuel, Philadelphia, Pennsylvania (13).

Prescott, William, Concord, New Hampshire (1).
Pruyn, J. V. L., Albany, New York (1).
Pugh, Evan, Centre Co., Pennsylvania (14).
Putnam, F. W., Salem, Massachusetts (10).

Q.

Quincy, Edmund, Jr., Boston, Massachusetts (11).

R.

Rankin, Alexander T., Buffalo, New York (15).
Ranney, Orville W., Buffalo, New York (15).
Rauch, J. H., Chicago, Illinois (11).
Raymond, R. W., New York, New York (15).
Redfield, John H., Philadelphia, Pennsylvania (1).
* Redfield, William C., New York, New York (1).
Rice, Clinton, New York, New York (7).
Richardson, Horace, Boston, Massachusetts (12).
Ritchie, E. S., Boston, Massachusetts (10).
Robertson, Thomas D., Rockford, Illinois (10).
Robinson, Coleman T., Buffalo, New York (15).
Rochester, Thomas F., Buffalo, New York (15).
Rockwell, Alfred P., New Haven, Connecticut (10).
Rockwell, John, La Salle, Illinois (11).
* Rockwell, John A., Norwich, Connecticut (10).
Rodman, William M., Providence, Rhode Island (9).
Rogers, Fairman, Philadelphia, Pennylvania (11).
* Rogers, James B., Philadelphia, Pennsylvania (1).
Rogers, W. A., Alfred Centre, New York (15).
Rogers, W. B., Boston, Massachusetts (1).
Rood, O. N., New York, New York (14).
Roosevelt, Clinton, New York, New York (11).
Rumsey, Bronson C., Buffalo, New York (15).
Rumsey, Dexter P., Buffalo, New York (15).
Russell, Andrew, Ottawa, Canada West (11).
Rutherford, Louis M., 175 2d Av., New York, New York (13).

S.

Safford, J. M., Nashville, Tennessee (6).
Safford, Truman H., Chicago, Illinois (13).
Sanborn, Francis G., Boston, Massachusetts (13).
Sargent, Rufus, Auburn, New York (10).
Scarborough, George, Sumner, Kansas (2).
Schanck, J. Stillwell, Princeton, New Jersey (4).
Schofield, J. M., Washington, District of Columbia (13).
Schott, Arthur C. V., Georgetown, District of Columbia (8).
Schott, Charles A., Washington, District of Columbia (8).

Scudder, Samuel H., Cambridge, Massachusetts (13).
Sellstedt, Laurentius G., Buffalo, New York (15).
Seward, William H., Auburn, New York (1).
Sexton, Jason, Buffalo, New York (15).
Seymour, M. H., Montreal, Canada (11).
Sheafer, P. W., Pottsville, Pennsylvania (4).
Shepard, C. U., Amherst, Massachusetts (4).
Sherwood, Albert, Buffalo, New York (15).
Sias, Solomon, Charlotteville, New York (10).
Sill, Elisha N., Cuyahoga Falls, Ohio (6).
* Silliman, Benjamin, New Haven, Connecticut (1).
Silliman, Benjamin, New Haven, Connecticut (1).
Sillsbee, E. A., Salem, Massachusetts (14).
Skinner, John B., Buffalo, New York (15).
Smith, A. D., Providence, Rhode Island (14).
Smith, J. L., St. Louis, Missouri (14).
* Smith, J. V., Cincinnati, Ohio (5).
Smith, James Y., Providence, Rhode Island (9).
* Smith, Lyndon A., Newark, New Jersey (9).
Snell, Eben S., Amherst, Massachusetts (2).
* Sparks, Jared, Cambridge, Massachusetts (2).
Spencer, Charles A., Brooklyn, New York (14).
Spring, Charles H., Boston, Massachusetts (13).
Squier, George L., Buffalo, New York (15).
Stanard, Benjamin A., Cleveland, Ohio (6).
Stearns, Josiah A., Boston, Massachusetts (10).
Stearns, William F., Oxford, Mississippi (13).
Steele, Oliver G., Buffalo, New York (15).
Steiner, Lewis H., Frederick City, Maryland (7).
Stevenson, Charles L., Charlestown, Massachusetts (14).
Stewart, William W., Buffalo, New York (15).
Stimpson, William, Chicago, Illinois (12).
Stoddard, O. N., Oxford, Ohio (7).
Storer, D. H., Boston, Massachusetts (1).
Storer, Frank H., Boston, Massachusetts (13).
Sullivant, W., S, Columbus, Ohio (7).
Swallow, G. C., Columbia, Missouri (10).
Swinburne, John, Albany, New York (6).

T.

* Tallmadge, James, New York, New York (1).
* Taylor, Richard C., Philadelphia, Pennsylvania (1).
* Teschemacher, J. E., Boston, [illegible] (1).
Thomas, Calvin F. S., Buffalo, [illegible]
Thompson, Alexander, Aurora, [illegible]
Thompson, Aaron R., New Y[illegible]

* Thompson, Z., Burlington, Vermont (1).
* Thurber, Isaac, Providence, Rhode Island (9).
Tillman, S. D., New York, New York (15).
Tingley, Joseph, Meadville, Pennsylvania (15).
Tingley, Joseph, Greencastle, Indiana (14).
Tolles, Robert B., Boston, Massachusetts (15).
Torrey, John, New York, New York (1).
* Totten, J. G., Washington, District of Columbia (1).
Townsend, Franklin, Albany, New York (4).
* Townsend, John K., Philadelphia, Pennsylvania (1).
* Troost, Gerard, Nashville, Tennessee (1).
Trowbridge, W. P., New York, New York (10).
* Tuomey, M., Tuscaloosa, Alabama (1).
Turner, Henry E., Newport, Rhode Island (14).
* Tyler, Edward R., New Haven, Connecticut (1).
Tyler, P. B., Springfield, Massachusetts (13).
Tyson, Philip T., Baltimore, Maryland (12).

U.

Upham, J. Baxter, Boston, Massachusetts (14).

V.

* Vancleve, John W., Dayton, Ohio (1).
Van Duzee, Mary K., Buffalo, New York (16).
Van Pelt, William, Williamsville, New York (7).
Vanuxem, Lardner, Bristol, Pennsylvania (1).
Vaux, William S., Philadelphia, Pennsylvania (1).
Verrill, A. E., New Haven, Connecticut (16).
Viele, Henry K., Buffalo, New York (15).
Vose, George L., Paris Hill, Maine (15).
Vought, John H., Buffalo, New York (15).

W.

* Wadsworth, James S., Genessee, New York (2).
* Wagner, Tobias, Philadelphia, Pennsylvania (9).
Wales, Torrey E., Burlington, Vermont (16).
Wales, William, Fort Lee, New York (15).
Walker, Joseph, Oxford, New York (10).
* Walker, Sears C., Washington, District of Columbia (1).
* Walker, Timothy, Cincinnati, Ohio (4).
Walling, H. F., Easton, Pennsylvania (16).
Ward, Henry A., Rochester, New York (13).
Warner, H. G., Rochester, New York (11).
Warren, G. K., Washington, District of Columbia (12).
* Warren, John C., Boston, Massachusetts (1).
Watson, James C., Ann Arbor, Michigan (13).

Watson, William, Cambridge, Massachusetts (12).
* Webster, H. B., Albany, New York (1).
* Webster, J. W., Cambridge, Massachusetts (1).
* Webster, M. H., Albany, New York (1).
Webster, Nathan B., Kenansville, North Carolina (7).
Wenz, J., New Orleans, Louisiana (15).
West, Charles E., Brooklyn, New York (1).
Wheatland, Henry, Salem, Massachusetts (1).
* Wheatland, Richard H., Salem, Massachusetts (13).
Wheatley, Charles M., Phœnixville, Pennsylvania (1).
Wheeler, T. B., Montreal, Canada (11).
Wheildon, W. W., Charlestown, Massachusetts (13).
White, Henry H., Harrodsburg, Kentucky (14).
Whitney, Asa, Philadelphia, Pennsylvania (1).
Whitney, H. H., Montreal, Canada (11).
Whitney, Josiah D., Cambridge, Massachusetts (1).
Whitney, William D., New Haven, Connecticut (12).
Whittlesey, Charles, Cleveland, Ohio (1).
Willard, Emma, Troy, New York (15).
Williams, Henry W., Boston, Massachusetts (11).
Williams, Matthew, Syracuse, New York (13).
Williamson, R. S., San Francisco, California (12).
Willmarth, A. F., New York, New York (15).
Wilson, Daniel, Toronto, Canada (10).
Winchell, Alexander, Ann Arbor, Michigan (3).
* Woodbury, L., Portsmouth, New Hampshire (1).
Worthen, A. H., Springfield, Illinois (5).
Wright, A. W., New Haven, Connecticut (14).
Wright, Chauncey, Cambridge, Massachusetts (9).
* Wright, John, Troy, New York (1).
Wurtele, Louis C., Acton Vale, Canada East (11).
Wurtz, Henry, New York, New York (10).
Wyckoff, C. C., Buffalo, New York (15).

Y.

Youmans, E. L., New York, New York (6).
* Young, Ira, Hanover, New Hampshire (7).

This list contains five hundred and thirty-one names, of which one hundred and three are of deceased members. The names of those who were chosen at Chicago, and who have already joined the Association, have not yet been incorporated into the general catalogue of members, but are printed separately on the following pages.

Doggett, Mrs. Kate N., Chicago, Illinois.
Doggett, William E., Chicago, Illinois.
D'Ooge, Martin L., Ann Arbor, Michigan.
Douglas, J. M., Chicago, Illinois.
Duncan, T. C., Chicago, Illinois.
Durham, Benjamin, Chicago, Illinois.

E.

Eaton, James H., Amherst, Massachusetts.
Eberhart, John F., Chicago, Illinois.
Edwards, J. B., Montreal, Canada.
Egleston, Thomas, Jr., New York, New York.
Ely, John F., Cedar Rapids, Iowa.
Embeck, William, St. Louis, Missouri.
Emery, Rush, Iowa City, Iowa.
English, William J., Ann Arbor, Michigan.
Everett, Oliver, Dixon, Illinois.
Everts, W. W., Chicago, Illinois.

F.

Farnsworth, P. J., Clinton, Iowa.
Farrar, Henry W., Cambridge, Massachusetts.
Ferry, William H., Jr., Chicago, Illinois.
Fisher, Davenport, Milwaukee, Wisconsin.
Foster, Henry, Clifton, New York.
Foster, John, Schenectady, New York.
Freeman, H. C., South Pass, Illinois.

G.

Gamgee, John, London, England.
Gaylord, Frederick, Chicago, Illinois.
Gill, Theodore, Washington, District of Columbia.
Goodwin, Daniel, Chicago, Illinois.
Gove, Edward, Portland, Maine.
Greeley, Samuel S., Chicago, Illinois.
Green, Oliver B., Chicago, Illinois.
Greene, Dascom, Troy, New York.
Greenebaum, Henry, Chicago, Illinois.
Grimes, J. Stanley, New York, New York.
Grover, Z., Chicago, Illinois.

H.

Hagen, Hermann A., Cambridge, Massachusetts
Hale, Edwin M., Chicago, Illinois.
Hall, P. A., Chicago, Illinois.
Halsey, Le Roy J., Chicago, Illinois.

Hamilton, Hugh, Harrisburg, Pennsylvania.
Hammer, A., St. Louis, Missouri.
Hammond, C. G., Chicago, Illinois.
Harris, Mrs. A. A., Cambridge, Massachusetts.
Harris, Clarendon, Laporte, Indiana.
Harris, Edward D., Cambridge, Massachusetts.
Harris, James W., Cambridge, Massachusetts.
Haven, Joseph, Chicago, Illinois.
Hawkins, B. W., New York, New York.
Hay, Walter, Chicago, Illinois.
Hayes, John L., Cambridge, Massachusetts.
Hendrick, Joel, Hornellsville, New York.
Hibbard, John R., Chicago, Illinois.
Hickcox, S. V. R., Chicago, Illinois.
Hildreth, Joseph S., Chicago, Illinois.
Hilgard, Theodore C., St. Louis, Missouri.
Hill, E. A., Chicago, Illinois.
Hilton, John C., Chicago, Illinois.
Hinrichs, Gustavus, Iowa City, Iowa.
Hitt, Isaac R., Chicago, Illinois.
Hobart, E. F., Beloit, Wisconsin.
Holmes, E. L., Chicago, Illinois.
Holstein, Charles H., Indianapolis, Indiana.
Horsford, M. L., Cambridge, Massachusetts.
Hoy, Philo R., Racine, Wisconsin.
Hubbard, Charles E., Boston, Massachusetts.
Hubbard, Gurdon S., Chicago, Illinois.
Hubbard, Sara A., Kalamazoo, Michigan.
Hunt, Charles S., New York, New York.
Hunt, W. C., Chicago, Illinois.
Huse, Frederick J., Evanston, Illinois.

J.

Johnson, H. A., Chicago, Illinois.
Judd, N. B., Chicago, Illinois.

K.

Katte, Walter, Chicago, Illinois.
Kedzie, J. H., Chicago, Illinois.
Keissell, A. S., Davenport, Iowa.
Klippart, John H., Columbus, Ohio.
Knickerbocker, Charles, Chicago, Illinois.
Koschkull, F. von, Tiflis, Russia.
Kost, J., Henry, Illinois.

L.

Lane, E. S., Sandusky, Ohio.

Doggett, Mrs. Kate N., Chicago, Illinois.
Doggett, William E., Chicago, Illinois.
D'Ooge, Martin L., Ann Arbor, Michigan.
Douglas, J. M., Chicago, Illinois.
Duncan, T. C., Chicago, Illinois.
Durham, Benjamin, Chicago, Illinois.

E.

Eaton, James H., Amherst, Massachusetts.
Eberhart, John F., Chicago, Illinois.
Edwards, J. B., Montreal, Canada.
Egleston, Thomas, Jr., New York, New York.
Ely, John F., Cedar Rapids, Iowa.
Embeck, William, St. Louis, Missouri.
Emery, Rush, Iowa City, Iowa.
English, William J., Ann Arbor, Michigan.
Everett, Oliver, Dixon, Illinois.
Everts, W. W., Chicago, Illinois.

F.

Farnsworth, P. J., Clinton, Iowa.
Farrar, Henry W., Cambridge, Massachusetts.
Ferry, William H., Jr., Chicago, Illinois.
Fisher, Davenport, Milwaukee, Wisconsin.
Foster, Henry, Clifton, New York.
Foster, John, Schenectady, New York.
Freeman, H. C., South Pass, Illinois.

G.

Gamgee, John, London, England.
Gaylord, Frederick, Chicago, Illinois.
Gill, Theodore, Washington, District of Columbia.
Goodwin, Daniel, Chicago, Illinois.
Gove, Edward, Portland, Maine.
Greeley, Samuel S., Chicago, Illinois.
Green, Oliver B., Chicago, Illinois.
Greene, Dascom, Troy, New York.
Greenebaum, Henry, Chicago, Illinois.
Grimes, J. Stanley, New York, New York.
Grover, Z., Chicago, Illinois.

H.

Hagen, Hermann A., Cambridge, Massachusetts
Hale, Edwin M., Chicago, Illinois.
Hall, P. A., Chicago, Illinois.
Halsey, Le Roy J., Chicago, Illinois.

Hamilton, Hugh, Harrisburg, Pennsylvania.
Hammer, A., St. Louis, Missouri.
Hammond, C. G., Chicago, Illinois.
Harris, Mrs. A. A., Cambridge, Massachusetts.
Harris, Clarendon, Laporte, Indiana.
Harris, Edward D., Cambridge, Massachusetts.
Harris, James W., Cambridge, Massachusetts.
Haven, Joseph, Chicago, Illinois.
Hawkins, B. W., New York, New York.
Hay, Walter, Chicago, Illinois.
Hayes, John L., Cambridge, Massachusetts.
Hendrick, Joel, Hornellsville, New York.
Hibbard, John R., Chicago, Illinois.
Hickcox, S. V. R., Chicago, Illinois.
Hildreth, Joseph S., Chicago, Illinois.
Hilgard, Theodore C., St. Louis, Missouri.
Hill, E. A., Chicago, Illinois.
Hilton, John C., Chicago, Illinois.
Hinrichs, Gustavus, Iowa City, Iowa.
Hitt, Isaac R., Chicago, Illinois.
Hobart, E. F., Beloit, Wisconsin.
Holmes, E. L., Chicago, Illinois.
Holstein, Charles H., Indianapolis, Indiana.
Horsford, M. L., Cambridge, Massachusetts.
Hoy, Philo R., Racine, Wisconsin.
Hubbard, Charles E., Boston, Massachusetts.
Hubbard, Gurdon S., Chicago, Illinois.
Hubbard, Sara A., Kalamazoo, Michigan.
Hunt, Charles S., New York, New York.
Hunt, W. C., Chicago, Illinois.
Huse, Frederick J., Evanston, Illinois.

J.

Johnson, H. A., Chicago, Illinois.
Judd, N. B., Chicago, Illinois.

K.

Katte, Walter, Chicago, Illinois.
Kedzie, J. H., Chicago, Illinois.
Keissell, A. S., Davenport, Iowa.
Klippart, John H., Columbus, Ohio.
Knickerbocker, Charles, Chicago, Illinois.
Koschkull, F. von, Tiflis, Russia.
Kost, J., Henry, Illinois.

L.

Lane, E. S., Sandusky, Ohio.

Lathrop, E. L., Chicago, Illinois.
Leakin, George A., Baltimore, Maryland.
Lincoln, Robert T., Chicago, Illinois.
Lombard, Benjamin, Chicago, Illinois.
Loring, Sanford E., Chicago, Illinois.
Lovering, James W., Cambridge, Massachusetts.
Ludlam, R., Chicago, Illinois.
Lunt, Orrington, Chicago, Illinois.
Lupton, N. T., Greensboro, Alabama.
Lyman, Henry M., Chicago, Illinois.

M.

McCagg, Ezra B., Chicago, Illinois.
McDermott, Michel, Chicago, Illinois.
McLain, B. W., Fort Wayne, Indiana.
McMurtrie, Horace, Boston, Massachusetts.
Magoun, George F., Grinnell, Iowa.
Mannheimer, M., Chicago, Illinois.
Marsh, H. H., Chicago, Illinois.
Marsh, James P., Chicago, Illinois.
Meehan, Thomas, Germantown, Pennsylvania.
Meeker, Arthur B., Chicago, Illinois.
Meysenburg, T. A., St. Louis, Missouri.
Miller, Joseph A., New York, New York.
Mitchell, William H., Florence, Alabama.
Moran, Charles, New York, New York.
Myers, W. H., Fort Wayne, Indiana.

N.

Nichols, Charles A., Providence, Rhode Island.
Norwood, J. G., Columbia, Missouri.

O.

Officer, Alexander, Chicago, Illinois.
Ogden, Mahlon D., Chicago, Illinois.
Ogden, William B., Chicago, Illinois.
Olmstead, Lemuel G., Moreau Station, New York.

P.

Page, Peter, Chicago, Illinois.
Park, Roswell, Chicago, Illinois.
Parkes, Charles T., Chicago, Illinois.
Peabody, S. H., Chicago, Illinois.
Peck, W. F., Davenport, Iowa.
Pennington, L. S., Sterling, Illinois.
Perkins, George H., New Haven, Connecticut.

Phillips, E. B., Chicago, Illinois.
Powell, Edwin, Chicago, Illinois.
Pratt, William H., Davenport, Iowa.
Prentiss, A. N., Lansing, Michigan.
Pumpelly, Raphael, Owego, New York.

Q.

Quinn, David, Chicago, Illinois.

R.

Read, Daniel, Alton, Illinois.
Read, Daniel, Columbia, Missouri.
Reed, Alexander, Philadelphia, Pennsylvania.
Reed, Caroline G., New York, New York.
Riley, Charles V., St. Louis, Missouri.
Robinson, Lucy, London, Canada.
Rockwell, Joseph P., New Haven, Connecticut.
Root, Edward W., Clinton, New York.
Rumsey, George F., Chicago, Illinois.
Rumsey, Julian S., Chicago, Illinois.
Russell, Jacob W., Chicago, Illinois.
Ryerson, Joseph T., Chicago, Illinois.

S.

Saunders, William, London, Canada.
Saunders, Mrs. William, London, Canada.
Sawyer, Charles B., Chicago, Illinois.
Scammon, J. Young, Chicago, Illinois.
Sheffield, Charles J., New Haven, Connecticut.
Sheldon, Edwin H., Chicago, Illinois.
Shepard, Charles U., Jr., Amherst, Massachusetts.
Skinner, E. S., Chicago, Illinois.
Smith, D. S., Chicago, Illinois.
Smith, James A., Chicago, Illinois.
Spalding, Jesse, Chicago, Illinois.
Sprague, Albert A., Chicago, Illinois.
Stallkneckt, F., New York, New York.
Stewart, Hart L., Chicago, Illinois.
Stockwell, John N., Brecksville, Ohio.
Stone, Samuel, Chicago, Illinois.
Storrs, Henry M., Brooklyn, New York.
Swasey, Oscar F., Beverly, Massachusetts.

T.

Tenney, Sanborn, Williamstown, Massachusetts.
Thompson, Harvey M., Chicago, Illinois.

Thompson, S. H., Hanover, Indiana.
Townshend, Norton S., Avon, Ohio.
Tracy, John F., Chicago, Illinois.
Trembley, J. B., Toledo, Ohio.
True, Nathaniel T., Bethel, Maine.
Tucker, William F., Chicago, Illinois.
Turner, John B., Chicago, Illinois.
Tuttle, Albert H., Cleveland, Ohio.

U.

Upton, George P., Chicago, Illinois.

W.

Wagner, William, Chicago, Illinois.
Walker, Charles H., Chicago, Illinois.
Walker, George C., Chicago, Illinois.
Walsh, Benjamin D., Rock Island, Illinois.
Ward, R. H., Troy, New York.
Warder, John A., Cincinnati, Ohio.
Warren, S. Edward, Troy, New York.
Welch, A. S., Iowa Agricultural College, Ames, Iowa.
Welch, Rodney, Chicago, Illinois.
Weyde, P. H. Van der, New York, New York.
White, Charles A., Iowa City, Iowa.
Whitford, W. C., Milton, Wisconsin.
Williams, Norman, Jr., Chicago, Illinois.
Wilson, Charles L., Chicago, Illinois.
Winslow, Ferdinand S., Chicago, Illinois.
Winslow, J. C., Danville, Illinois.
Wood, Horatio C., Jr., Philadelphia, Pennsylvania.
Wood, John H., Chicago, Illinois.
Woods, John, Philadelphia, Pennsylvania.
Woodworth, John M., Chicago, Illinois.

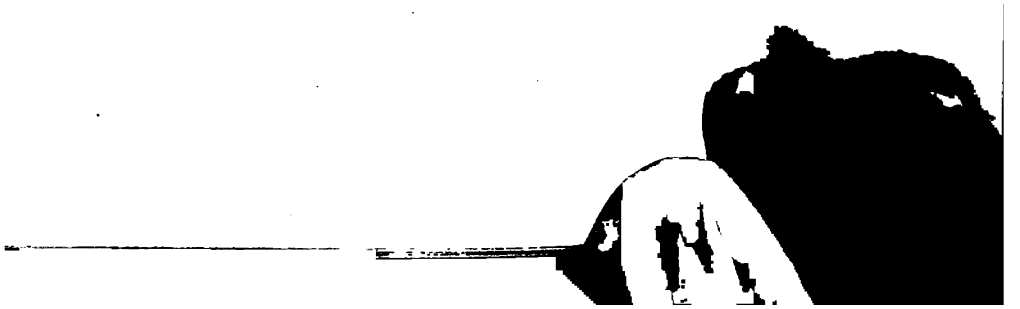
Y.

Young, John Jay, Chicago, Illinois.

Z.

Zaremba, Charles W., Chicago, Illinois.

NOTE.—Whole number of new members two hundred and fifty-eight.



ADDRESS

IN COMMEMORATION OF

ALEXANDER DALLAS BACHE,

BY

BENJAMIN APTHORP GOULD.

GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—

IT is no common event which you have called upon me to commemorate. Death, who comes with an impartial tread into the hovels of the poor and the castles of monarchs, is a visitor too well known to us all. Yet, sometimes he assumes a new aspect. When, three years ago, at the very moment of the hard won and stern rejoicing of the Republic, we saw her first citizen struck down in the midst of the people who honored him, the destroyer gained almost a new power in our imaginations. So is it with us now. He, who by common consent, unquestioning and unchallenged, stood forth preëminent as our leader in science, our first counsellor where her welfare was at stake, unflinching in the maintenance of her interests, wise in the guidance of her affairs,—he has gone from among us.

Adequately to portray the character, abilities and influence of such a man would be an undertaking from which the ablest tongue or pen might shrink. It is not for the labor it implies, for labor is fitly bestowed in recording a life at once so great and so inspiring; not from the natural timidity, which even the most competent might well feel, in presuming to pronounce any judgement upon so rare a virtue, ability and patriotism; nor is it even that our point of view is palpably too near, to permit the just portrayal of this lofty character with all the truthfulness of outline for which a comprehensive survey of the whole in its

many relations is imperative; or to give its fair proportions, undistorted by the mists which encompass our vision, and untinged by the hues with which affection adorns his image. Beside all these, there is yet another reason for distrust. His influence extended through so large a sphere that it is difficult for us fully to comprehend it now. The more we examine the tokens of its action, the more do we become impressed with its extraordinary range. I know of no department of physical or natural science which has not been stimulated or fostered through his means. The legislative and executive departments of the nation knew his power through many years, and relied upon it in matters far beyond the range of his ordinary pursuits. Both the army and navy felt, and have often acknowledged their obligations to him. The progress of education, the development of scientific research, the extent of scientific discovery, the growth of the arts, and the spread of commerce, have all been greater in America because he has lived.

Such a man was our beloved and honored BACHE. To hesitate, when summoned to put into words our common tribute, were unworthy of his friend or of his pupil. You will all feel the inadequacy of the offering, and the futility of attempting to compress into the utterance of an hour or two such records and such results. Many of you have already brought him a better tribute,—that of years of fruitful labor, prompted and encouraged by himself, or of a change in the aims and pursuits of a lifetime, induced by his wise and kindly counsel.

ALEXANDER DALLAS BACHE was born in Philadelphia on the 19th day of July, 1806. If intellectual and moral eminence might be inferred from an honorable lineage, which, unhappily, is not always the case, it certainly would have been anticipated for him. His father was the son of Richard Bache and Sarah Franklin; the former, President of the Republican Society of Philadelphia at the outbreak of the American Revolution, and Postmaster General of the United States from 1776 to 1782; the latter, the only child of Benjamin Franklin and his wife Deborah (Read), and herself eminent as one of the heroines of the war of independence. The noble women who, during our recent struggle for the maintenance of the nationality then achieved, gave their time and energies to the support of their

countrymen facing the edge of battle, and who have aided in making the name of our own Sanitary Commission immortal, were but the unconscious imitators of that smaller but equally devoted band who, during our first struggle for national existence, similarly labored in mitigating the sufferings of our soldiers. Like them, Mrs. Bache ministered to the sick and wounded in army hospitals; and under her superintendence more than 2200 women were at one time employed in making garments for the barefooted and half-clad men, who, against almost unparalleled obstacles, were achieving the independence of a continent and a new vantage-ground for the oppressed of all nations. The maternal grandfather of our departed colleague, for whom he received his name, was Alexander James Dallas, Secretary of the Treasury of the United States from Oct. 1814 to Nov. 1816, who, in the second war of our Republic, redeemed its finances from confusion, and, in the short space of two years, restored them to a condition of order and stability. Under his administration also, the Coast Survey of the United States was established, and Mr. Hassler appointed to its superintendence. The late Commodore Dallas, and Mr. George M. Dallas, Vice President of the United States, were his sons.

A peculiarly large number of Mr. Bache's family, both on the paternal and maternal side, were engaged in the government service, civil or military, and young Bache was destined by his father for the army, which offered also to the boy an attractive career. Accordingly, at the age of fifteen, he entered the Military Academy at West Point, where he graduated, July 1, 1825, the first scholar in a class so far above the usual grade of excellence that four of its members were assigned to the Corps of Engineers, although for more than one or two in each class to attain this distinction was a rare occurrence. Among his classmates at West Point were General Robert Anderson, of Fort Sumter, and Major General C. F. Smith, who served his native land so faithfully and effectively during the recent rebellion.

It is recorded of Bache that, during the whole term of his course at the Military Academy, he never incurred a single mark of demerit. And this is all the more to his credit, in that

he was no demure and prematurely sedate youth, but possessed, in an eminent degree, that love of frolic and of jest which formed a prominent trait of his character in riper years. Nor was the high position which he acquired in the confidence and respect of his instructors attained at the expense, in any degree, of the affection of his fellow pupils. The bonds of friendship formed within the precincts of the Military Academy seem to have been of remarkable strength, and were most tenderly guarded by him throughout his subsequent life.

The final examination of the cadets, just before the close of their academic course, was attended then, as now, by prominent officers of the military service. In the year now referred to, the Secretary of War himself was present, and the tokens of scholarly and military excellence given by the first pupil of the class were such as to elicit the most uncommon marks of approval. The Secretary himself, with a sympathetic appreciation which does him the highest honor, was prompted to the unusual step of addressing a letter to Bache's mother. This letter, one of that mother's most cherished mementoes, I am permitted to read to you.

WEST POINT, June 10th, 1825.

My Dear Madam:—Upon any other occasion than the subject of the present letter, I should be obliged to admit that our very small acquaintance would rebuke me for addressing you. But, being a father, and knowing how exquisite is the pleasure arising from the well-doing of children, I am quite sure, from your amiable disposition, that you will forgive me, when you learn that my only purpose in writing is to state, as I do most sincerely, how greatly I was gratified at the evidences given by your son in his examination, of the excellence of his attainments. He ought to be, as I am sure he will be, a source of the greatest consolation. I know not whether it has been your lot to have your cup of life drugged in any degree with calamity. The draught must have been severe indeed if it is not sweetened by the blessing of your excellent son. I knew and loved your father—his great paternal ancestor I knew only by his works. I thought, or permitted myself to believe, that I saw the excellences of both branches about to be united in your son. I offer you my sincere congratulations.

JAMES BARBOUR.

MRS. SOPHIA BACHE.

That mother's cup of life was indeed bitterly drugged with calamity; but the supporting arm of her son, the rich honors

which she happily lived to see accorded him in no stinted measure, and the abundant benefits to the commonwealth and to the nation, which followed his accession to each successive place of influence or authority, did most effectively sweeten it, and cause it to run over with gladness.

During the first year after graduating, he remained at West Point, as assistant to Professor Mahan in the department of Engineering, and, in the summer of 1826, was detailed as assistant to Colonel Totten, then in charge of the construction of Fort Adams, at Newport. Here commenced a close friendship between these two eminent men, which increased with their increasing years, and was severed only by death. And when, long years afterwards, their duties brought them once again to the same place of residence, and permitted a resumption of their intimate communion, the joint influence of General Totten, as Chief of the Engineer Corps, and of Professor Bache, as Superintendent of the Coast Survey, quietly but steadily wrought a wondrous change in the welfare of scientific interests, and in the position of scientific men at Washington. In their intimate domestic intercourse they not only occupied the relation, but assumed the titles of kindred; and to their mutual support in times of peril to the great intellectual interest which they defended, American science will be forever indebted.

Nor was General Totten the most important or the nearest friend whom Bache won for life during his sojourn of two years at Newport. It was his privilege there also to enlist the affection and secure the hand of an admirable woman, who, for nearly forty years, accompanied him in all his many wanderings; by her sound judgement, unsurpassed devotion, and intellectual ability, multiplying his opportunities of usefulness as well as his happiness, and rendering it possible for him to accomplish, for his own honor and for the welfare of his country, what no man probably could have accomplished without some such assistance.

On the 16th of September, 1828, only three years after graduating from West Point, and only two years after the commencement of his professional duties at Fort Adams, Mr. Bache was elected Professor of Natural Philosophy and Chemistry in the University of Pennsylvania, at Philadelphia. He was at

this time but twenty-two years old, yet the repute of his career at West Point had been already a source of pride to his native city, and sundry communications, which he had found time in Newport to contribute to the *Mathematical Journal*, had borne witness to his mental activity and ability. Mr. Bache obtained a six months' leave of absence, was married as soon as it was received, and, on the 11th of October, about three weeks after his election, arrived in Philadelphia to assume the duties of his professorship. These duties proving acceptable to himself and to the college authorities, he definitely resigned his position in the army at the expiration of his leave of absence.

Here commences his scientific career, interrupted sometimes by the pressure of other duties, but never discontinued, no matter what his avocations, so long as God granted him and us a continuance of his mental powers.

For seven years Professor Bache retained his position in the University, beloved by his pupils, esteemed by his colleagues, respected by the whole community. During these years he was an active member of the Franklin Institute and of the Philosophical Society, and their transactions contain some twenty-five contributions from him within this period, all of them recording the results of original scientific research.

Among these papers some deserve especial mention.

The earliest which I find recorded is an article "On the Specific Heat of the Atoms of Bodies," published in February, 1829. In this firstling he maintained that the best and latest determinations of the atomic weights and specific heats of elements failed to support the doctrine that the specific heat of the atoms is the same for all bodies. The topic was a large one for discussion by a young man of twenty-two, but he certainly made his point good; and if the theory which he then opposed has found acceptance in subsequent years, it has only been because the data upon which it rests have been modified by more accurate determination.

This paper was followed, a year later, by his first experimental research, which was upon the inflammation of phosphorus in a vacuum, or rather, in a highly rarified medium. This appeared in the *American Journal of Science*, bearing date May, 1830, and was the beginning of a more extended line of investigation,

subsequently followed up. A stage was reached, at last, where it became requisite to learn the order of thermal conducting power for various powders, since the research depended on the degree to which that small amount of heat could be retained, which was generated by means of what little oxygen remained for contact with the phosphorus. In the air many powders were capable of thus producing inflammation; and exhaustion of the air operated simply by removing a conveyer of heat. These conducting powers were not determined, and the research remained unpublished.

The influence of the Franklin Institute, in giving to Bache's first researches an especially practical character, is very noticeable at this period. General Mechanics received a large share of his attention. An article which he printed in April, 1831, proposes a safety-apparatus for steamboats, composed of a combination of the French plan of plates of fusible metal, with the ordinary safety-valve, so that on the melting of the plate the supply of steam might not be totally lost, thus obviating the main objection to the employment of fusible metal. A year later, he proposed an alarm to be applied to the interior flues of steam boilers. In this, the ordinary difficulties attending the use of fusible metal for such purposes are obviated, by inclosing it in a tube, and imbedding in it the lower end of a rod, to the upper end of which is applied some retractive force. The fusion of the metal releases the rod, causing a bell to ring, or a whistle to sound; after which the rod may be restored to its place, and the metal allowed again to solidify around it. Thus the metal is not exposed to the contact or pressure of the steam, but only to its heating agency. Experiments tried with this apparatus a year or two later, by the Committee of the Franklin Institute on Explosions of Steam Boilers, led to a very favorable report, accompanied by the suggestion that it is equally applicable to boilers without interior flues, and might be employed as a manageable and useful check in ordinary cases upon the safety-valve.

In the beginning of the year 1832, Mr. Saxton, who was then in London, wrote to friends in Philadelphia an early account of Faraday's discovery of magneto-electricity, and of his own repetition of the experiments, as well as those of Nobili made

soon afterwards. Apparatus was immediately constructed for repeating the experiment, both in the English and Italian form, and careful observations were published by Professor Bache in the Memoirs of the Franklin Institute for July. His inferences were most philosophically and accurately drawn, and entirely accord with the laws now so well known. What a field has been traversed in the progress of our physical knowledge since that not very distant day! The production of a spark by magnetic agency was a vast step toward making possible the quantitative demonstration of the Correlation of Physical Forces. But Saxton, in writing, carefully says, "a spark resembling the electric spark;" and Bache's first inquiries were directed to solving the great question whether it were indeed electrical in its nature. A quarter-century later, and these sparks, "resembling the electric spark," were flashing messages of peace and good will to men between the home of Faraday and the home of Bache.

I pass over his translations of important recent physical and chemical memoirs, which did good in their day by bringing the newer results of science home to our doors, at a time when familiarity with modern languages was comparatively a rare accomplishment, and so also his experiments on the Navigation of Canals by Steam; but must comment, for an instant, on the interesting and valuable results of his experiments on the correctness of measurement of rainfalls made by the ordinary gauges. These observations were made in 1833, although not published until five years later, when he communicated them to the British Association at its meeting in Newcastle, which he attended. These experiments were very labored, and led to interesting deductions. He placed rain-gauges at each corner of Beck's shot tower, in Philadelphia, at a height of 162 feet from the ground. The locality was favorable, since Philadelphia is situated upon an extensive plain, where neither the falling rain nor the wind encountered purely local influences. The observations were made at first at three different heights, the original motive of the investigation having been, to determine the extent to which the amount of rain collected is affected by the height of the station. It soon became manifest that the effect of eddy-winds was greater than that of the difference of

elevation; and Professor Bache himself became convinced that the variation in quantity of rain collected at different heights was chiefly due to this previously unsuspected source of error. The quantities of rain collected at the different angles of the tower varied very greatly; and, in one extreme case, during a westerly wind, the quantity in the gauge at the south-east angle was $2\frac{1}{3}$ times as great as that in the north-west gauge. In general, the leeward gauge received more than the windward one. And since the heavier rains generally fall in connection with certain particular directions of the wind, it is clear that averages derived from observations of these rains are liable to constant errors, in the determination of the quantity of rain falling at a given height from the ground. Regarding the supposed variation in quantity as dependent upon variation in height, the results were far less marked; and, although the temperature of the rain-drops was often found higher than that of the air through which they were falling, he states that he never saw a case in which the gauges at the top of the tower showed more rain than those below. The extensive details of this research have not been printed. It was far from Bache's usage to send his papers abroad for publication, but these results were, with entire propriety, communicated to the eminent foreign scientific body whose sessions he was attending, a welcome and honored guest. Speaking of them in later years, he says: "The experiments have never been published *in extenso*, because I thought the cream of them had been taken off, and there was no use for the skim-milk!"

But the subject which, not only at this period but throughout his life, seemed to possess for him the greatest attraction was Terrestrial Magnetism, then made especially prominent as a field for physical investigation, through the experimental and theoretical discoveries of Gauss and Weber. As early as the year 1830, he had equipped a little magnetic observatory in the garden attached to his house, in which regular observations were made during a period of four or five years. In his journeys he carried with him portable instruments with which he determined the magnetic constants of the points visited. What he accomplished in later years for this favorite branch of science, the world knows; and it is certainly not too much

to say that, of what we know to-day of the distribution, intensity, and periodic and secular changes of terrestrial magnetism, we are indebted quite as much to Bache as to any other one man.

His first memoir on the subject was presented to the American Philosophical Society in November, 1832, and contains the results of hourly observations of the declinations, in which he was assisted by Professor Frazer. These observations were made with a very long needle, provided with a graduated arc at each end. Beside some small notices of the observed influence of Auroras upon the needle, a phenomenon regarding which facts were then wanting, he published, in the Transactions of the Philosophical Society, in connection with Professor Courtenay (at that time his colleague in the University of Pennsylvania), two very elaborate memoirs on the Dip and Horizontal Intensity of Terrestrial Magnetism at several places in the United States. The care and thoroughness with which these observations were arranged, and the reductions effected, could with difficulty be surpassed.

The illusive promises of brilliant results, so familiar to all students of the physical sciences, and which present such temptations to premature inferences, were not wanting in these magnetic investigations; but Bache, though a young man, was truly a philosopher,—as cautious in his deductions as he was keen in following up the track of an unusual phenomenon or the varied suggestions to which renewed experiment might afford a clue. These earlier observations exhibited an apparent connection between the weather and the amount of variation of the needle. Even the approach of a summer shower seemed to be announced by changes in the magnet. On the morning of the great meteoric display in 1833, Mr. Bache thought he had detected an unusual disturbance,—the needle, which was generally kept in the house, having been carried out to the observatory for greater convenience. But the same transfer, on subsequent mornings, produced the same effect, an effect afterwards referred to changes of temperature; and instruments within doors gave, through several seasons, different results from those observed out of the dwelling-house. Whether the changes of temperature occasioned an alteration in the mag-

netic axis of the needle, or to what other agency the result was attributable, remained undecided.

During the solar eclipse of 1834, before he had received the Gaussian apparatus, he carried out a careful series of observations to ascertain whether the magnetic forces were disturbed during the eclipse. For these observations he employed the long declination-needle already described, a horizontal bar vibrating in a rarefied medium, and a dipping-needle poised on a knife-edge. Writing of it nine years later, he says: "I had nearly cried 'Eureka' once, but had occasion afterwards to doubt." And again, in a tone almost of sadness, he adds: "The ordinary labors in magnetism are like those in astronomy; they yield no point of discovery, but go to the general accumulation of facts." To what extent he aided in such accumulation in later years, you know.

The observations of magnets during auroral displays promised, at the time, important results; and Faraday encouraged him to proceed in the same course. The formation of an auroral arch appeared to bring the declination-needle to a "point"; but the difficulties arising from the conflict of the testimony of different instruments were found very serious. To obviate these, he resorted to the mode of observation already alluded to, in which the needle was suspended in an atmosphere rarefied to the pressure of from three to three and one-half inches of mercury, a method described in the memoir cited, and which proved of importance for the light bars of Hansteen and others, before Gauss had shown the importance of heavy bars, and thus brought about their general use. Of this same memoir he makes a remark, in the letter to Professor Henry, from which I have already quoted, which may touch a sympathetic chord in the hearts of some of the seniors before me, who recall the intellectual position of the American investigator at epochs by no means so remote as this, of thirty-two years ago.

"This memoir," he says, "has been voted prolix. The reason for my making it in such detail was a sneering remark, that American observations were not given with the necessary detail to enable observers in Europe to judge of their value. Perhaps I erred in being so circumstantial. The neglect with which my friend, Major Sabine, treated these observations, on the plea that I had not combined the dip with them, and that they were over too limited an extent of coun-

try, hurt me much; and, in a controversy before the British Association, I charged upon him without drawing bridle, and with a *temporary* effect. The dip observations I was not satisfied with, at the time, having exaggerated ideas of the accuracy attainable in such things. When I saw other instruments and observations, I found that I had been unnecessarily fastidious."

Early in 1833, he published an edition of Sir David Brewster's Treatise on Optics, with Notes and an extended Appendix, giving the mathematical investigation of subjects concerning which the author had only given the results, in popular form.

In May, 1833, a bill having been introduced into the Legislature of Pennsylvania establishing and regulating the standard of Weights and Measures, then not fixed by the Federal government, the House of Representatives referred the bill to the Franklin Institute for their opinion. The Institute referred the matter to a special committee of nineteen, of which Mr. Bache was chairman. Their report was prepared by him, as well as a special analysis of the various reports previously made on Weights and Measures in the United States.

This report is worthy of especial note, since it doubtless contributed to hasten that desirable consummation, the establishment, by Congress, of a uniform standard of Weights and Measures, for which the power had been expressly delegated by the constitution. It took the strongest ground in favor of such Congressional action.

"So impressed are the committee with this view," says the report, "that they would express it as their decided opinion that the most imperfect system of Weights and Measures which has ever been framed, would, if applied in all the States of our Union, be preferable to the most perfect system which should be adopted by any one commonwealth singly. . . . Indeed, in most of the laws of more recent origin adopted by the several States, there is a distinct provision that when Congress shall furnish a system of Weights and Measures for the United States, the temporary State standards shall be made to conform to the national standard. The exceeding importance of uniformity is thus distinctly set forth from quarters of the highest authority in the different parts of our extended republic."

The report, which was signed by all the members of the committee, and unanimously adopted by the Board of Managers of the Institute, concluded by recommending that in-

dependent action on the part of the State be suspended for a season, but that the attention of Congress be especially called to the importance of fixing the standard of Weights and Measures.

Before long, Congress had taken the much needed action, and established uniform national standards, probably the best attainable without the introduction of new names and ideas; and before the expiration of a single decade from the date of this report, Mr. Bache himself occupied the position of Superintendent of Weights and Measures for the United States.

Like the great body of scientific investigators throughout the world, he felt the convenience and many advantages of the metric system, and fervently looked forward to the time when that uniformity, for which the individual States of America and of continental Europe were then earnestly striving, should find its application on a grander scale, and when all civilized nations, scorning the prejudices which rivers, oceans or mountains might hem in, should join in one measure, one weight, and one coin; deeming this apparently far off, yet not impossible, attainment to be not merely an efficient source of immediate benefit, but also a great step toward hastening on that happy era, sung by poets and foretold by prophets,—

> "When the war-drum throbs no longer, and the battle-flags are furled
> In the parliament of man, the Federation of the world."

The only hinderance to his immediate advocacy of the metric system arose from a fear that the nation was not yet ripe for it, that new names would alarm the illiterate, and that premature efforts for its introduction might defeat their own end. Still he retained the meter as the fundamental unit for all the measurements of the Coast Survey; and in all discussion and official action, whether in the United States Office of Weights and Measures, in his many relations to the Mint, or as Chairman of the Committees upon Weights and Measures in this Association and in the National Academy, his policy always had reference to that desirable epoch, which must surely come, when the metric system shall be thoroughly introduced into the United States. How would his heart have swelled with joy could he have foreseen that, even now, while I speak to you, this has been established as the standard of Weights and Meas-

ures for more than half, and legally authorized for two-thirds of the population of continental Europe; adopted by one-half of that of South America; that the United States have not only put it on the same legal footing as the old systems, but have introduced it into their domestic and foreign postal arrangements, and are issuing coins of metric dimensions and weight, for the purpose of disseminating among our people an acquaintance with the system; that Russia has declared her readiness to follow, when the United States shall have also joined the long list of nations who have established it in good earnest; that China, at once the eldest and the youngest of the family of nations, is on the eve of establishing the same standards; and that, even in conservative England, its use has been authorized by statutory enactment; so that the elegant and harmonious system of meters and grams now stands in the same legal category with avoirdupois, troy and apothecaries weight, with beer-measure and wine-measure, cords, acres, stones, quarters of various sorts, and all the other arbitrary and incongruous denominations, which nothing but hereditary acquisition could save from the verdict of utter absurdity; divided too, as they are, into parts of 3, 4, 5½, 6, 8, 12, 14, 16, 31½, or whatever else they may be, certainly not decimal.

Two very extended investigations, in which Mr. Bache bore an important part, ought not to be omitted from mention here. The first is the well-known and elaborate Report upon Water Power, by a committee of the Franklin Institute, a document prepared by Mr. Bache, although he took no part in the published portion of the experiments. The other is the Report made in 1836, on the Explosions of Steam Boilers, which cost him some years of hard work, and is signed by him alone in behalf of the committee of the Franklin Institute. It is elaborate and learned, and rendered essential service in promoting judicious legislation. The causes of explosions are considered under five general divisions, comprising all the causes at that time recognized. Yet it is a most instructive consideration for the student that we are now, after the lapse of thirty years, aware that one of the most serious sources of danger arises from a cause at that time unknown: namely, the sudden evolution of steam in consequence of temporary diminution of pres-

sure, and, perhaps, the mechanical agitation, produced by lifting the safety-valve, — the pressure of this steam, when disengaged, surpassing that which had previously sufficed to confine it in the liquid form. Happily, this source of danger also has now been obviated, by means of a principle closely analogous to that, which at this time, formed the subject of another of Mr. Bache's investigations, and regarding which his experiments led to an unfavorable report: namely, the principle of circulation in the water of a boiler. The celebrated Jacob Perkins, seizing, with the quick perception of genius, upon this principle, which had long been employed in the bleacheries of continental Europe, undertook, by introducing into the boiler an inner lining, open at the bottom, to confine the generation of steam to the annular space between the boiler-wall and this false lining, thus causing an upward current within this space, maintained by the water descending through the central core. The theory seemed good, but Bache's experiments showed that, far from greatly promoting the generation of steam, such small influence as was exerted by the inner cylinder, was in the opposite direction.

Now that we have the additional knowledge of thirty years on the subject, we easily perceive the reason. The water foamed in the annular space, not because it did, but because it did not, circulate freely. Had the water in the inner vessel been unable to receive any heat which had not traversed the outer one, and had the ratio of the sectional area of the inner vessel to that of the annular space been twelve or fifteen times greater than it was, a rapid circulation would have been established; foaming would have disappeared; the rapidity of formation of steam would have been increased three fold; and the water would have remained in such a condition that no diminution of incumbent pressure could occasion a sudden evolution of steam.

In the same year he made a thorough survey of the track and the effects of a severe tornado which had visited the vicinity of the city of New Brunswick, and obtained quantitative, as well as qualitative, determinations of its effects. The results here, as, likewise, those of similar subsequent surveys of tornadoes near Newark and near Philadelphia, tended to confirm the then

new and unpopular theory of Mr. Espy, of an inward motion near the surface of the earth.

But I am dwelling far too long upon the manifold labors of this preliminary period of Bache's scientific career. To present an analysis of his labors during those seven years, would be an agreeable task; but far more than a proper portion of this discourse has already been demanded by the variety of their scope. Ingenuity, mental activity, interest in all branches of the physical science of the day, and a zealous industry that shunned no toil, characterize this early period of his career, as emphatically as that of his maturer life. Six chemical papers lie before me; among them are an analysis of anthracite coals, made jointly with Professor H. D. Rogers; experiments on the corrosion of metals by salt water; on the action of different alkalies in conferring hydraulic properties upon lime, and on a new method in alkalimetry. Here, too, are nine others, yet unmentioned, upon subjects in thermics and meteorology; among them, three historical notes, 1st, on the date of Dr. Franklin's discovery that our north-east storms begin at the south-west, fixing this date satisfactorily in 1743; 2d, on the received hypothesis to explain the greater quantity of rain which falls at the surface of the ground than above it; and 3d, on the discovery of the non-conducting power of ice; the origin of both these latter being conclusively traced to his great ancestor. There is a charming little paper describing experimental illustrations of various phenomena in thermics. There are methods of recording the force and direction of the wind, subsequently introduced with benefit into the Coast Survey. There are meteorological observations, and observations of meteors, simultaneously with Mr. Espy, for determining their height, and with Professors Henry and Alexander, for determining longitude, these last being the first successful ones made for this purpose; and there is one of the most noted of all his memoirs, the investigation of the Influence of Color upon the Absorption and Radiation of non-luminous Heat.

The basis of these experiments was the principle, that for each substance there is a thickness beyond which radiation from it is constant, so that their radiating powers are rightly compared, not by equalizing either their thickness or their weight,

but by determining, for each substance, what the thickness in question is. Obtaining then a variety of pigments, of the same color differing chemically, and of kindred chemical composition differing in color, he undertook to determine their different powers of absorption and radiation; as, also, the modification of these powers, which changes of color, occasioned by chemical means, would produce. The principle in question was established, but no connection between color and radiation was detected.

I pass from this interesting period of Bache's life with an allusion to one more of his works, at that period eminently characteristic of the man, his polemical discussion with Olmsted regarding the periodical recurrence of meteors. Mr. Bache maintained that there was no recurrence in 1834; Professor Olmsted, on the other hand, maintained the reverse. Professor Bache instituted special inquiries at the military posts, where of course sentinels were on duty, along all the frontiers of the United States; also among the night-police of various cities, and at the Universities; and he found but one exception to the statement that no unusual number of meteors was seen. Of this controversy Bache wrote, in 1846:

"There is something yet to be found out on this subject, which may reconcile our opinions. Neither I, nor any of those watching with me, or for me, have seen an unusual number of meteors on the night of the 12th of November, in any year since the great night at Philadelphia, and we have taken great pains to be sure. Yet, I cannot doubt the testimony as given for some other places. . . . I had a complimentary letter from the Professor in regard to my manner of conducting the controversy, which I valued more highly than if I had gained the victory."

That "something yet to be found out" we now know, thanks to our colleague, Professor Newton, and to the brilliant celestial confirmation of the correctness of his computations on the night of November 13–14, 1866 and 1867. It affords the anticipated reconciliation of the two opinions, while the history of that controversy illustrates the firmness, mildness, charity and courtesy of our lost friend and leader.

Thus earnestly, variedly and successfully prosecuting his physical investigations, surrounded by a genial circle of friends, whose companionship he always loved to recall, enjoying the

affection of his pupils in the University, filling an important position in the management of the Philosophical Society and of the Franklin Institute, winning for himself an honored name on both sides the Atlantic, dwelling with his kindred, their stay and staff,—his busy life went on, till suddenly a new path of usefulness opened before him, and led to an interruption of the whole tenor of his thoughts and ways.

Stephen Girard had died in 1832, leaving an immense sum of money for the endowment of a college for orphans. In 1833, Professor Bache had been elected one of the Trustees of the college, and on his thirtieth birthday, July 19, 1836, he was, by the unanimous vote of the other Trustees, appointed President of the Institution. On accepting this new position, so full of promise, he received instructions to visit Europe, in order to examine similar establishments there, and thus to procure the means of opening the college under the most favorable circumstances. Accordingly, resigning his post in the University of Pennsylvania, he prepared for an immediate departure, and sailed for Europe September 30, 1836.

During two years happily and profitably passed in visiting the principal countries of Europe, he formed many close friendships among the leading investigators of physical science, while actively prosecuting the especial objects of his mission; and moreover found opportunities for a determination of the magnetic dip and intensity at twenty-one European points, with the same apparatus and by the same methods which he had employed in America, thus rendering it possible to combine the results of both investigations, without danger of introducing extraneous errors. These objects once accomplished, he returned in October, 1838, in the hope and expectation of insuring an early opening of the college. During the winter of 1838–9, he prepared his "Report on Education in Europe," a large octavo book of 666 pages. Of the contents of this elaborate volume, I will not enter into any detailed analysis before the Association. By the universal consent of those best competent to judge, it enjoys the reputation of a master-piece. The fruits of an examination of about 280 schools, comprising examples of all the principal educational institutions below the grade of Universities in Great Britain, France, Switzer-

land, Holland, Italy, and the several German States, are here systematically presented; though the work claims only to be an abstract of the more prominent parts of the mass of documentary matter brought home by Mr. Bache, and arranged under his direction, so that, to use his own words, it might "always throw the light of experiment upon doubtful points in the working of our system of organization. In many cases the documents descriptive of the schools give the actual results of suggestions contained in the works on education."

The history of the delays in the opening of the Girard College, of the vast sums and almost interminable time expended on the construction of the building, and of the various other difficulties, which seemed full of unhappy auguries, needs no repetition here. No effort was spared by Professor Bache to accelerate the opening of the institution. We have seen that he hastened home immediately on the completion of his labors in Europe, notwithstanding the cordial recommendation of the Trustees, that he should remain. At the beginning of his report he says:

"It is almost needless to say, that I am now not only ready, but anxious to render available as soon as possible, in the organization of the Girard College, the knowledge which has thus been acquired. A due execution of the instructions of the scholastic committee required not merely an examination of orphan houses and elementary schools, but of the various modes of education and grades of instruction. . . . I must be allowed to say, that in the course of attempting its execution, I have spared no personal exertion; and though I may regret it was not in abler hands, my conscience acquits me of having wasted any parts of the time and means so liberally placed at my disposal by my fellow-citizens."

A year passed by; the imposing structure, in which the orphans were to receive their education, remained unfinished; Professor Bache's salary continued; but there was no work for him to do. His mind was filled with the ideas and experiences on educational matters, which had accumulated during three years of continuous application to inquiries in this direction. Being therefore alike unwilling, by resignation of his office, to abandon the bright prospects which his enthusiasm had portrayed for the future of the college, or to receive a salary for which he was rendering no manifest equivalent, he conceived

the post of offering his services gratuitously to the city of Philadelphia for the purpose of reorganizing its High School, then languishing under many important reverses. His offer was accepted, and he received and assumed the position of Principal of the High School, which he retained for more than a year, effecting during that period a complete reformation of the school, which soon assumed position among the best in the land. Finding then that the college was still not in a condition to promise any speedy commencement of its activity, he turned his energies anew, although against the remonstrances of the Trustees, to the service of the Nation. He resumed, however, the nominal office of President, and kept himself ready to assume its active duties whenever opportunity should offer. Meanwhile he retained from the city authorities the joint position of Professor of the High School and Superintendent of Public Schools, together with a salary which, though but little more than half that which he resigned, was a liberal one for those days. This position he retained for nearly two years, until, in July, 1843, three years and a half after his return from Europe, he was reappointed to his old position in the University, and left the city-schools. His reoccupation of the Professorship was, however, of short duration, for in November, 1843, he received the appointment of Superintendent of the Coast-Survey, and here, at the age of thirty-seven years, began the great and crowning work of his life.

During the five years since his return from Europe, though his time and energies were greatly drawn upon by the exigencies of the educational duties, his scientific labors had been by no means intermitted. He had published his observations of the magnetic dip and intensity at twenty-one stations in Europe. He had organized a magnetic observatory at the Girard College, and raised the means for the prosecution of observations during a series of years, which demonstrated for the first time that the small changes of magnetic declination in Europe and America are not similar. He had invented an ingenious instrument for determining the dew-point, which is even now probably the best one available for that large class of cases, where the observations must be made by men not specially trained to scientific investigation. A series of com-

parative observations, with this instrument and the wet-bulb hygrometer, for about two years, is among the records of the Girard College Observatory. Long afterwards, Bache found that Belli of Milan had anticipated him in the general principle upon which the instrument depends. "The form of my instrument," said Bache, "is quite different from his, and I think it has advantages enough to entitle me to keep the name, but alas, principle was the soul, and I do not care much to dispute for the dead body."

The principle consists in cooling one end of a conductor below the dew-point, while the other is at the temperature of the air. The intermediate parts then assume different temperatures, and some one section will have the precise temperature of the dew-point. The conductor is quicksilver, contained in a highly polished metallic tube, on which the marginal line of deposition of dew becomes sharply marked. A thermometer, plunged in the mercury at this point, indicates, of course, the degree of temperature sought. The details of contrivance in this instrument are admirably managed; and, although doubtless the neat device of Regnault for attaining the same result, by the evaporation of ether from a thimble, furnishes a more accurate method in the hands of a philosopher, it may be doubted whether, in those of a mechanical observer, it would be found preferable.

During the same period he had introduced a modification of Osler's anemometer, avoiding the friction of the apparatus necessary for guiding the spring, as also the exposure of the spring to the weather and to great fluctuations of temperature. He had made a series of numerous experiments with Fourier's thermoscope of contact, in order to determine the conducting powers of powders and tissues, hoping thus to remove the obstacles which had prevented the farther prosecution of the subject of his first published paper, that on the inflammation of phosphorus. But finding these results unsatisfactory, he had invented a new thermoscope of contact better adapted to the purpose, and capable, by a slight modification, of being applied to a determination of the conducting powers of liquids.

And, besides these, he had carried on his investigations upon heat to a very considerable extent, and although the results

the idea of offering his services gratuitously to the city of Philadelphia, for the purpose of reorganizing its High School, then lamentably deficient in many important respects. His offer was accepted, and he received and assumed the position of Principal of the High School, which he retained for more than a year, effecting during that period a complete reformation of the school, which soon assumed position among the best in the land. Finding then that the college was still not in a condition to promise any speedy commencement of its activity, he insisted on resigning his salary, although against the remonstrances of the Trustees. He retained, however, the nominal office of President, and held himself ready to assume its active duties, whenever opportunity should offer. Meanwhile he received from the civic authorities the joint position of Principal of the High School and Superintendent of Public Schools; together with a salary which, though but little more than half that which he resigned, was a liberal one for those days. This position he retained for nearly two years, until in July, 1842, three years and a half after his return from Europe, he was reappointed to his old position in the University, and left the city-schools. His reoccupation of the Professorship was, however, of short duration, for in November, 1843, he received the appointment of Superintendent of the Coast-Survey, and here, at the age of thirty-seven years, began the great and crowning work of his life.

During the five years since his return from Europe, though his time and energies were greatly drawn upon by the exigencies of the educational duties, his scientific labors had been by no means intermitted. He had published his observations of the magnetic dip and intensity at twenty-one stations in Europe. He had organized a magnetic observatory at the Girard College, and raised the means for the prosecution of observations during a series of years, which demonstrated for the first time that the small changes of magnetic declination in Europe and America are not similar. He had invented an ingenious instrument for determining the dew-point, which is even now probably the best one available for that large class of cases, where the observations must be made by men not specially trained to scientific investigation. A series of com-

parative observations, with this instrument and the wet-bulb hygrometer, for about two years, is among the records of the Girard College Observatory. Long afterwards, Bache found that Belli of Milan had anticipated him in the general principle upon which the instrument depends. "The form of my instrument," said Bache, "is quite different from his, and I think it has advantages enough to entitle me to keep the name, but alas, principle was the soul, and I do not care much to dispute for the dead body."

The principle consists in cooling one end of a conductor below the dew-point, while the other is at the temperature of the air. The intermediate parts then assume different temperatures, and some one section will have the precise temperature of the dew-point. The conductor is quicksilver, contained in a highly polished metallic tube, on which the marginal line of deposition of dew becomes sharply marked. A thermometer, plunged in the mercury at this point, indicates, of course, the degree of temperature sought. The details of contrivance in this instrument are admirably managed; and, although doubtless the neat device of Regnault for attaining the same result, by the evaporation of ether from a thimble, furnishes a more accurate method in the hands of a philosopher, it may be doubted whether, in those of a mechanical observer, it would be found preferable.

During the same period he had introduced a modification of Osler's anemometer, avoiding the friction of the apparatus necessary for guiding the spring, as also the exposure of the spring to the weather and to great fluctuations of temperature. He had made a series of numerous experiments with Fourier's thermoscope of contact, in order to determine the conducting powers of powders and tissues, hoping thus to remove the obstacles which had prevented the farther prosecution of the subject of his first published paper, that on the inflammation of phosphorus. But finding these results unsatisfactory, he had invented a new thermoscope of contact better adapted to the purpose, and capable, by a slight modification, of being applied to a determination of the conducting powers of liquids.

And, besides these, he had carried on his investigations upon heat to a very considerable extent, and although the results

were not, in his opinion, sufficiently definite and complete to justify their publication, still his expressions of hope that he might yet resume and complete these researches were ever earnest.

I cannot refrain from making known the reason of their discontinuance.

One room on the sunny side of his house was appropriated to these experiments; the various thermoscopes and all the subsidiary apparatus were arranged there, and the apartment was held sacred to scientific investigation. One evening, while he was attending a session of the Philosophical Society, an alarm of fire broke out in the neighborhood. His mother, then a member of his family, heard the alarm, and hastily entered the room without a lamp, to look from the front window. A crash reminded her, too late, of the inconsiderateness of her movements. The apparatus was entirely destroyed, and the first words which greeted her son on his return told him what he had lost. He made no reply, but went to the room and silently surveyed it. The destruction was complete, and the hard labors of nearly a year were rendered fruitless. An eye-witness has described it to me. He stood white with emotion for a few moments; then, turning away, only trusted himself to say that he would return soon, and hurried out of the house. Half an hour in the open air restored him to himself; returning, he consoled his mother, and made light of the occurrence; nor did he ever afterwards explain the reason why his observations on heat were discontinued.

It was in November, 1843, that Mr. Bache was appointed to the Superintendence of the Coast-Survey, his appointment to the Office of Weights and Measures following, a month later. The volume of testimonials and recommendations, upon the strength of which this appointment was made, has been shown me; and their number and character has made a deep impression. I cannot believe that such a weight of recommendation was ever brought at any time in support of a candidate for office, on purely intellectual grounds. I can think of no man in the country, eminent in physical science or holding a prominent scientific position, whose name was not signed to some one of that voluminous mass of memorials, asking the appoint-

ment of Professor Bache. All the scientific societies and colleges, together with several of the learned associations of Europe, gave their influence and added their indorsement to the request. How such a unanimous declaration on the part of experts could have failed of success, it were difficult to conceive; assuredly it was tenfold more honorable than any mere appointment. But to all these was added a yet more effective influence, growing out of the personal and political relations which by good fortune were brought to bear, and which it was impossible to resist. The President, Mr. Tyler, issued the commission, in spite of the avowed and vigorous opposition of the Secretary of the Treasury, Mr. John C. Spencer, under whose immediate direction the Coast-Survey belonged.

So far as the earnest and unanimous support of the lovers of science in the country was concerned, (science had then few, if any, real votaries other than those who were incidentally enabled to prosecute their studies in the intervals of other professional labor,) Mr. Bache entered upon his new duties with confidence. But he found his position one in which such moral support could afford but little aid. Internal disorganization, insubordination and dissension embarrassed him on the one hand; and the distrust and unconcealed animosity of the Secretary shackled him on the other. These were circumstances to evoke and make manifest the true greatness of the man.

Mr. Hassler, his predecessor, was a man of high attainments and ability, whose scientific management of the work, which he had himself initiated, had won universal approbation. He had emigrated to this country from Switzerland at the very beginning of the century, and had brought with him ideas of scientific accuracy and thoroughness, which the public mind in America was not yet sufficiently enlightened to appreciate, or even to understand. He gave to the Survey the chief energies of his life, and, undeterred by its suspension for nearly fifteen years, resumed its prosecution, when permitted anew, with the same zeal which had marked its inception. In Mr. Bache's words, "For his successful struggle against great difficulties, his adopted country will, no doubt, honor his memory, as the

pioneer in a useful national undertaking." On the other hand, he was a man of great eccentricity of manner, and not endowed with administrative ability. Science was at a low ebb among Americans when he came to our shores, and but few received the benefit of the scientific knowledge which he brought with him. Excepting Nicollet, himself a foreigner, and Dr. Patterson of Philadelphia, Hassler appears to have little acquaintance, and still less personal intercourse, with learned men in the United States. The scientific development of the country seems to have gone on unperceived by him; and, to quote the words of one who knew him, "he died in the belief that the nation as a whole, was, in 1843, where he had found it in 1801, so far as its science was concerned." Comparatively few native Americans obtained employment on the Survey under him, and one of his surveying party has informed me that he himself was the only man of that party who spoke English habitually. His scientific affiliations were exclusively transAtlantic, and while he seems to have entertained a sort of general affection for his adopted country, he apparently looked upon Americans as necessarily tyros in scientific matters, and deemed the refinements and elegancies of the higher Geodesy matters entirely beyond their comprehension. When on one occasion he had applied for an increase of salary, the Secretary of the Treasury, upon whom he was urging his claim, is said to have replied, "Why, Mr. Hassler, that is as much as I receive myself;" to which with charming frankness he replied, in strong Swiss accent, "What if it is! Any President can create a Secretary of the Treasury; but only the Almighty can make a Superintendent of the Coast-Survey."

"At the time of Hassler's death," I quote the language of a distinguished officer of that period, "the condition of the Coast Survey was anomalous and Ishmaelitish. Every man's hand was against his neighbor. Hon. John C. Spencer, the Secretary of the Treasury, was the real head of the Survey; and the principal assistants reported directly to him, and not to Mr. Hassler, who was thus reduced to the position of nominal Superintendent, but the real Chief only of the primary triangulation party."

Mr. Spencer was, as I have said, strongly opposed to Mr.

Bache's appointment. He favored the claims of the senior assistant. The two assistants, next in order of appointment advocated the same, desiring to see the principle of succession by seniority established. Other assistants urged yet other men, but none favored Mr. Bache. After his appointment, four of the eight yielded their coöperation, as did a fifth after a time, but the others were as ingenious as they were active, in devices for establishing and maintaining a system of petty persecution. Never was magnanimity more grandly exhibited than in Mr. Bache's course, while making himself Superintendent in fact as well as in name; but on this there is no need to dwell. Illustrations of his greatness of spirit are wanting at no period of his career.

Perhaps I may best illustrate the conciliatory power of Mr. Bache, and the magnanimity of Mr. Spencer, by reading to you a letter, written by the latter at the time of his resignation, being only five months after Mr. Bache had taken charge of the Survey in opposition to his wishes. It is addressed to Professor Bache, and forms a fitting pendant to the other letter, already laid before you.

TREASURY DEPARTMENT, May 1st, 1844.

Sir: — I am unwilling to leave this Department without communicating to you the great pleasure I have derived from the intercourse which has subsisted between us since your appointment as Superintendent of the Coast Survey; and my conviction of the great service you have already rendered the country in the arrangements made for carrying on that work. The system, order and regularity to which you have brought the complicated and difficult operations of that great work, afford the strongest assurance that it will now proceed with vigor and despatch, as well as with economy. My thorough knowledge of all your difficulties, plans and improvements, derived from the intimate communications that have been maintained between us, justify me in saying, that in my opinion the work could not be entrusted to more capable and judicious hands than yours. I shall look forward with great anxiety to the accomplishment of those great results which I am confidently anticipating in the successful prosecution of your very laborious and highly responsible undertaking.

With great respect and esteem,

Your friend and servant,

Prof. A. D. BACHE,
Sup't Coast Survey.

J. C. SPENCER.

In these five short months the moral and intellectual power of Bache had not only triumphed over the obstacles which the Secretary mentions, but over the numberless misrepresentations to which he had at first lent an ear; and finally over the Secretary himself, whose manliness forbade him to retire to private life without first leaving a public record to remedy any previous injustice. He had regarded and described Bache as a "mere college professor," without practical administrative ability, or any special qualification for the Coast-Survey. But he saw and acknowledged the error, and remained his fast friend until death.

Six months thus passed in establishing authority, and in learning the precise condition of the work which he had undertaken to prosecute, and regarding which the continued ill will of the three senior assistants and the chaotic condition of its affairs, had at first thwarted his endeavors.

But at last, freed from the influences of internal disorder and dissension,—having gained the confidence of the Treasury Department, having established discipline with the three insubordinate assistants, without any manifestation of unkindness on his own part, and having secured the respect and cordial regard of the other officers of the Survey,—Professor Bache was able to apply his energies with effect to the development and prosecution of the great undertaking before him.

The first step was to enlarge the scope of the work, avoid sectional jealousies, and accelerate its execution, by dividing the coast into sections, in each of which a base-line should be measured and the triangulation extended in opposite directions from this base to the confines of the section, where the work of the adjacent one would meet it; each thus testing the other. The advantages thus gained in soothing prejudices, already serious in those States which the connected primary triangulation of his predecessor had not yet reached, was greater than I can describe, and conduced in no small measure to the public favor and support; which his tact and discretion, even more than his scientific power, soon won for the Coast-Survey. Within the first year, the operations of the Survey were carried on in nine States of the Union; within the second, in twelve; within the third, in fourteen, notwithstanding the withdrawal of all the army officers, on account of the Mexican

war; and within the fourth year in seventeen,—the Atlantic and Gulf coast having been divided into nine sections, a division still retained.

The first season was in a great degree devoted to the trial of instruments and methods. The thirty-inch theodolite was, and is to this day, a truly serviceable instrument, but the astronomical apparatus belonged to a by-gone era. To Lieutenant T. J. Lee, and to Mr. Boutelle, both of them gentlemen of experience in practical observation, was assigned the duty of testing the instruments available for latitude-determinations, the chief of which was a Borda's repeating Circle. After a summer of faithful and repeated trials they reported that their best results for latitude had been obtained with the vertical circle of a six-inch Gambey theodolite, which they had borrowed. There were three transit-instruments, two of them five feet long, and the third and best of the three, only two feet. All proved unequal to the demands of geodetic astronomy in 1844, and were soon replaced by instruments of more modern construction. Observations of the dip and variation of the needle, and of the intensity of terrestrial magnetism, were introduced as part of the regular routine. So too were observations of the tides. The employment of solar observations for determining azimuths was exchanged for observations of the pole-star at both elongations. The form of geodetic signals invented by Mr. Borden, and used by him in the trigonometrical survey of Massachusetts, was substituted for that previously used; and is still regarded, by common consent, as the most accurate and convenient form ever devised.

As rapidly as means allowed, the services of American scientists throughout the land were enlisted in aid of the Survey, and the whole intellectual resources of the country thus made tributary to its usefulness and success. Thus Walker, Peirce, Bailey, Agassiz, Barnard, Kendall, Mitchell, Bond, Alexander, and many others were called on to assist in the advancement of the undertaking; and this large and wise policy prevailed during the whole period of his superintendence. Various other institutions took the name "national," but there was only one really national scientific institution for the first twenty years of his administration, and that was the United States Coast-Survey.

As the work gradually expanded under his guidance, many of the ablest officers of the Army and Navy were brought into the Coast-Survey service. They found an opportunity for the use and development of their powers which otherwise might long have remained wanting, and gladly served under a civilian who manifested such a knowledge of men, and such a capacity for using them. It is surprising to see how many of our ablest military and naval officers found employment, and attained distinction, in the Coast-Survey. Of the army, I need only mention the present Chief of the Engineer-Corps, General Humphreys, the worthy follower of Totten, Generals Stevens, Foster, Prince, Cullum, Cram, Hunt, — so too Johnston and Hill, whose intellectual powers were then enlisted in their country's service. Of the navy, among others, — Vice Admiral Porter, the two Rodgerses, Davis, Gilliss, Sands, Craven, Ammen and Luee. During our late war, a large majority of the most useful commanding officers of the navy had served upon the Coast-Survey, and they did not hesitate to assert that their experience there had been of the highest service to them in the naval duties of the war.

In the second year of Bache's administration the base-apparatus was perfected by him, the Zenith Telescope first employed in the Coast-Survey for the determination of latitude, and the exploration of the Gulf-stream commenced. In the third, the magnetic telegraph was first introduced as a regular geodetic method for the determination of longitude, and a special party organized under Mr. Walker for the prosecution of longitude-operations. In the fourth year, a chronometic expedition was organized for the better determination of the transAtlantic longitude, and the method of star-signals adopted for telegraphic measurements, — a method "destined," to use Walker's own words to this Association, "sooner or later to perfect the geography of the globe." The system of magnetic and tidal observations had then attained an extended development; and arrangements were already in progress for the measurement of two arcs of the meridian, and one of a parallel of latitude.

I will not undertake any detailed narration of the successive stages in the development of the Survey under Professor Bache, but a few words may be deemed not amiss regarding

some of the more marked steps in its progress. The apparatus for the measurement of base-lines, devised by Professor Bache in 1845, and constructed by Würdemann, whose services had even at that early day been secured for the Coast-Survey, and to whose exquisite skill and ingenuity many of the details are due, attained a degree of excellence before unknown. It can be safely asserted that with this apparatus, under Bache's skilful manipulation, the practical sources of error became less than the theoretical, or, in other words, that a line could be measured with such precision that the uncertainty arising from all errors of every kind, incurred in the measurement, was less than the uncertainty in the length of the standard of measurement. This wondrous accuracy was attained by the introduction of two new principles, both due to Professor Bache.

Owing to differences of specific heat and conducting power, different metals subjected to the same circumstances, do not change their temperatures with equal rapidity. Consequently an apparatus constructed for perfect compensation by the joint employment of brass and iron, on the principle of the gridiron pendulum, may not be correctly compensated, so as to remain of unvarying length during a change of temperature, which is in fact the very condition which naturally exists at the time of use. By numerous experiments Bache succeeded in so arranging the cross-sections of the bars, that while the extent of their surfaces remained the same, their masses should be inversely as their specific heats, allowance being made for their conducting powers. The same varnish being applied to the surface of each, in order to confer equal absorbent capacity, the compound bar must then retain the same length, not merely at all temperatures, but during all changes of temperature.

The other principle which he introduced consists in the substitution of actual for optical contact, thus dispensing with the use of microscopes, a practical gain of great value in the field. The contact-level of Bessel finds here an appropriate application; the agate plane, carried indirectly by the compensation lever which connects the brass and iron at their free ends, abuts against an upright lever on the other bar, which transmits the pressure to the contact-level.

A description of this exquisite apparatus was given to this

Association at its Washington meeting by our lamented colleague, the late Major Hunt of the United States Engineers. At Bodie's Island in North Carolina, Professor Bache measured a base-line six and three-fourths miles long, in ten working days, with a total probable error of less than one tenth of an inch. On one day at this time, 1692 meters, or one and a sixteenth miles, were measured in eight and a half hours. In measuring the base on Epping Plains in Maine, 8716 meters long, the probable error committed was less than sixteen millimeters; and the probable error of any distance in the primary triangulation of the New England States is to its total length, as 1 to 288000.

The determination of latitudes with the Zenith Telescope by Talcott's method was first tested in 1845, and proved so far superior to any other known means of determination possible in the field, that it was adopted by Professor Bache for the Coast-Survey operations. I need not tell you of the excellences and precision of this method, for it is now more than twenty years since the attention of astronomers was first called to it, while the annual Reports of the Coast-Survey, and numerous communications to scientific periodicals in this country, have furnished continual illustration of its preëminent merit. And in a recent number of the Proceedings of one of the leading scientific societies of Europe, published in this very year, 1868, attention is called to the Coast-Survey method as a very excellent one, and well deserving of a trial! Here, too, as with the base-apparatus, a degree of accuracy is attainable in observation, surpassing that afforded by the fundamental data; so that not the errors of instrument or observer, but those of the star-declinations, limit the correctness of our results. To Bache we owe the recognition and adoption of this transcendent method, and to him also those refinements of process and improvements of apparatus, by which alone its accuracy is rendered possible.

To the telegraphic determination of longitudes, and the development of the method of star-signals, I have already alluded. The latter was soon found applicable, with almost equal advantage, to the regular observation of transits in fixed observatories, and all the apparatus devised for the one purpose answered with equal aptitude for the other.

The simple and easy process of making telegraphic comparisons of clocks, whose respective errors are determined by local observation, was early discarded in spite of its tempting facility, since the apparent accordance of results thus obtained soon showed itself illusive. Bache's rule in the Coast-Survey was that all the scientific work should be executed in the most thorough and accurate manner which the resources of science and art would permit. He never shunned a tenfold labor, if it was to be repaid by double precision, accepting the great principle which prescribes a higher rate of effort as we climb to higher degrees of refinement. Nor did another great law which the history of science has taught from the beginning, and is teaching now, fail of its continual application and illustration; just as it will not fail hereafter, unless all inferences from experience are futile. This law ordains that the conscientious investigation of truth for its own sake shall be rewarded by some unforeseen practical benefit. The struggle to attain, at whatever effort it may cost, all possible accuracy throughout those investigations whose value is dependent on their precision, finds unexpected recompense in new clues to phenomena before unknown, and in the disclosure of new laws. Thus it was that the method of star-signals led to the method of telegraphic registration, now universal in observatories.

This important advance in practical astronomy received its full development in the Coast-Survey. There, long years ago, its methods were brought to the same degree of refinement and completeness of detail, which they have but recently attained in Germany. The precautions requisite in observing, the study of personal equations both in noting the transits and in reading the records, the modes by which the clock can best be made to graduate the time-scale, the various forms of chronograph for maintaining that time-scale, — all sprang from the Coast-Survey, and received their full development either from the regular offices of the Survey, or from others acting temporarily as assistants. Among the earliest results of this method as applied to the measure of longitudes were the discovery that the time required for the transmission of signals by electric telegraph was appreciable, and therefore measurable, and the determination of

their velocity. These investigations and results were of course not all of them due to Bache directly or alone, but none the less were they due to him. Not merely to the general expansion which he gave to the operations under his direction; not merely to the large policy, according to which he secured astronomers for astronomical, physicists for physical, and mathematicians for mathematical, research. These would indeed have been ample grounds on which to claim for him a large share of the honor due for valuable discoveries and successful applications. But in fact his personal relation to the results was far closer. He knew, understood and guided, even when one or more other minds were active between him and the results. Accomplished himself in practical observation, skilful in experiment, thoroughly acquainted with the progress of all the larger investigations prosecuted by others under his direction,—he was no administrative officer in that too frequent employment of the term, which dispenses with the understanding of work directed to be done by others, and actually makes the words "administrative" and "executive" to denote opposite qualities. Day by day, step by step, in the successive detection of principles, elaboration of methods, discovery of facts, he was in constant intercourse with the investigator,—stimulating his zeal, encouraging his hope, suggesting new ideas or infusing needful caution. Yet with unfailing magnanimity he never claimed in such cases any personal honor for success, nor did he disown any personal responsibility for failure. May I be allowed to repeat a sentence which I addressed to this Association fifteen long years ago, when we mourned together over the loss of Walker, and you had called on me to recount his honorable deeds? In speaking of the method of star-signals, I then said:

"Mr. Walker has informed me that this suggestion was due to the Superintendent of the Survey, but its practical application seems to have been a result in the elaboration of which the two bore an equal part. At least I may be permitted to state the still more honorable fact, that in the very many conversations which it has been my privilege to hold with each of the two gentlemen separately upon this interesting question, their descriptions varied but in one salient point, namely, that each ascribed the chief merit to the other."

Thus by the use of the Zenith Telescope, combined with the determination of longitudes from the adopted meridian by the

exchange of star-signals, the geographical position of the primary astronomical stations of the Survey could claim, ten or fifteen years ago, to be determined with more accuracy than that of any European observatory.

The temptation is strong to recount the similar advances made through Bache's administration of the Coast-Survey, in almost every department of physical science. Not only Geodesy and Astronomy were thus made gainers by new methods and implements of research, but other sciences were similarly promoted and their advancement stimulated.

Stations for tidal observation were established all along the Atlantic, Gulf and Pacific coasts. Self-registering tide-gauges (the invention of the ingenious Saxton, whose invaluable services Bache early secured, and retained to the last), were brought into extensive use. Our knowledge of tidal phenomena and laws assumed new proportions, and by unwearying persistence in questioning, the secret of many a mystery in their complicated action was extorted. The character of the ocean currents along our coasts was determined, and their causes elicited. All along the shores of the Atlantic, Pacific and Gulf of Mexico, beyond the lines of soundings, the deep-sea lead, and the deep-sea thermometer were busily plied by the exploring vessels of the Survey, while the immediate coast was fringed with a net-work of soundings, — the deep-sea lead, thermometer and sounding lines all being at the same time essentially improved. Twice was Agassiz sent to study the coral reefs of Florida, to discover the method of their formation and the laws which promote and restrict their growth. The most accomplished students of the infusoria were kept supplied with specimens from the dredge and sounding line for their microscopes. The magnetic instruments were improved, and the magnetic constants determined for every important point possible, within the reach of the Survey.

The exploration of the Gulf-stream, commenced in 1844, was vigorously prosecuted, its temperature at the several depths determined, and its structure and laws for the first time detected. The cold wall of water between the Gulf-stream and the shore, as also the division of the stream proper into alternate bands of warmer and cooler water, were discovered, meas-

ured and mapped out for the benefit of navigators and the use of scientists. So too were a series of nearly parallel ranges of hills at the bottom of the stream, the most westerly being 1500 feet high, and the whole forming a series of corrugations corresponding with the bands of warm and cold water. The facts that the general direction of the stream is governed by the configuration of the bottom and that the temperature varies from year to year, thus standing in an important relation to the meteorological phenomena of the coast, were also detected. These discoveries were, in a large degree, made by Mr. Bache individually, upon a study of the observations, although no small share in them justly belongs to Professor Trowbridge. In the year 1860, fourteen sections of the stream had been surveyed, 300 positions upon them occupied, and 3600 observations of temperature made.*

But I must not continue in this strain. That Bache touched no question of science which he did not adorn with new discoveries and new means of attaining yet farther ones,—that the range of the Survey was made to cover almost the whole range of physical science, from the structure of the microscopic dwellers in the bed of the ocean, up to the improvement of

*During Professor Bache's Superintendence of the Coast-Survey, the positions of 9816 geodetic stations were trigonometrically determined.

4989 triangulation-stations were occupied.
267 magnetic-stations were occupied.
68 base-lines were measured, being 150 statute miles in total length.
102 longitudes were astronomically determined.
151 latitudes " " "
92 azimuths " " "
12850 square miles of area were topographically surveyed.
38850 " " " " " hydrographically "
7222449 soundings were taken.
7500 specimens of bottom were obtained and preserved.
1080 tidal stations were occupied.
133 shoals and reefs were discovered and determined.
41 important channels were discovered and their changes determined.
1020 current stations were occupied.
970 topographical snap-sheets were constructed.
877 hydrographical " " "
270222 sheets of charts were printed.

In addition to these the positions of many hundreds of isolated rocks, ledges and shoals were for the first time accurately determined.

The party directed by Professor Bache in person occupied, during the same period, 49 primary triangulation-stations, measured 6 base-lines, of which the aggregate length was 33 statute miles, and determined astronomically 5 longitudes, 29 latitudes, 31 azimuths.

lunar tables and the determination of positions of fundamental stars,—you all well know.

I have said that to him the scientific progress of the nation is indebted, more than to any other man who has trod her soil. Nor was this bold statement made in forgetfulness or ignorance of the great debt we owe to many illustrious men. Nor do I fear that any of you who know his services will think it overstrained or ill-considered. I call you to witness that it is true; that not his great ancestor who outvied Prometheus, not the statesmen who have guided the legislation of the republic, not all the educators, who have shaped the policy of its colleges and schools, not all the great masters in physics or mathematics, zoölogy or geology, have so effectively, widely or intensely stimulated the advancement of science in America, as Bache did through the agency of the Coast-Survey. What was the branch of physical science, which was not called upon to minister to the Survey, or which failed to receive an impulse from it in return? Had the chief object of this work been the promotion of scientific research, it could not have been more effectively aimed at, or attained.

Addressing a scientific body on the scientific career of our departed leader, it is only from this especial point of view that I have considered his administration. But that would be a very one-sided presentation of the facts, and one eminently unjust to his memory, which should leave on a single mind the impression that the scientific researches or influences, of which I have spoken, were attained at the sacrifice, or disregard in the slightest degree, of those important practical purposes for which the Coast-Survey was established and supported. So too would it be unpardonable to omit, from the recital of his services and achievements, the work he did for commerce, for the national defence, and for the development of national resources. All this he accomplished the more thoroughly and the more economically, in that he called to his aid the resources of the highest science. Although embarrassed by difficulties peculiar to our own country and offset by few corresponding advantages, his survey of the Coast of the United States may challenge comparison, as regards its completeness and thoroughness, with any geodetic or hydrographic work

ever executed. The ocean currents have been explored with untold gain to our seamen, the variation of the needle, and its rate of change, determined with unsurpassed precision along the three seaboards of our country, the tidal laws so well determined that the tide-tables published by the Survey for many years have been found conformable with observation to a degree previously unattained in any part of the world, the heights of mountains and the depths of oceans measured and mapped out,—and all this accomplished at a cost inferior, almost beyond comparison, to that of similar surveys in other countries. As has been well said by Professor Trowbridge, the annual cost of the Survey, at the time of its most extended activity under Mr. Bache, was but little more than the cost of a first-class steamship. It never reached an annual cost approaching the price of some of the floating palaces which ply upon Long Island Sound, or up the Hudson. Nor was there probably a single year of the twenty-three years of Bache's superintendence, when the discovery of reefs, shoals, currents, channels or rocks, would not have been cheaply purchased by the nation, at an outlay of all the survey ever cost. So much it may not be amiss to say upon this purely practical point, since, though of course needless for you, it might not be so for others to whose eyes these words may come.

But his benignant influence was not alone exerted through the agency of the Coast-Survey, with which his name is, and forever will be, associated. Can any of us forget his relation to this Association, of which he was one of the founders? No man surpassed him in efforts for its success. Never absent, never without some tangible proof of interest and good wishes, —his counsel, more than that of any one man, was sought on all questions of policy, and his opinion received with unsurpassed respect. Over three of the fifteen sessions of the Association held during his life, he presided, and hither he brought the annual contributions of his abundant discovery.

So was it also with the National Academy, the complement and coadjutor of this Association. There too he was a founder, the most active member, and its President. And but for the bitter dispensation which cut him off amid his usefulness and honor, his well-known voice would have sounded in your

ears, calling on both institutions to stand firm by one another, mutually supporting, mutually dependent, and capable, by joint action and reciprocal confidence, of accomplishing fourfold what either could do alone.

So too was it with the Smithsonian Institution. From the beginning he was a Regent, and to his active support and earnest endeavor, more than to that of any other, we owe it that this Institution, under its present honored head, has been able to adopt and to maintain a policy, which shall make it what its founder intended: "an institution for the increase, and the diffusion of knowledge among men." It is safe to say that, but for Bache, Professor Henry would not have assumed the Secretaryship, nor have been willing to retain it; and but for Bache, his wise policy would in all probability have failed of full adoption.

No scientific undertaking, indeed, failed to receive from Mr. Bache hearty sympathy, personal assistance, and whenever circumstances warranted, official support. Gilliss's Expedition to Chile, Kane's and Hayes's Polar expeditions,—all of which, like many others on a minor scale, he served with his counsel, his influence, his official assistance and his purse,—illustrate his anxiety to aid the extension of human knowledge.

But it was not merely by his ardent love of science, and disinterested devotion to her welfare that he accomplished so much. His fertility of device, unconquerable assiduity, large policy, generous impulses, patriotic devotion, might well have coexisted without yielding such fruits in the development of the Coast-Survey, or such a mighty power for good in the promotion of science throughout the United States. More than these was needed; far more than these he possessed. It would be safe to say that the greatest of all his mental gifts or attainments were his marvelous knowledge of human nature and his unrivaled skill in using it. He had studied men, as he once expressed it to me, as he would study physical phenomena. To a faculty of persuading the most obstinate, of soothing the most irritable, of encouraging the most disheartened; to a power of stimulating the indolent, controlling the impulsive, winning over opponents by the charm of his manner, and confirming friends by the truthfulness and sincerity of his nature,

—he added that rare endowment, which imbued others unconsciously with his own zeal. His was what might be called a magnetic nature, for his companionship evoked latent aspirations, and pointed to noble aims. It was to his personal, more even than to his scientific, qualities, that the Coast-Survey owed the recognition of its importance by Congress and the people, and the annual provision made for its maintenance. He knew the secret of obtaining work from his subordinates, by doing more than they did. His desire to accomplish much himself led him during half the year into the field, where he in person performed the most difficult geodetic operations; thus insuring the accuracy of the portion accomplished by himself, and maintaining such an intimate acquaintance with the practical details as enabled him to understand the real needs of those engaged in similar operations elsewhere. Annually he made tours of inspection through all the sections accessible to him. He took a personal share in the details of all the explorations; and no assistant, permanent or temporary, felt, in entering on a new field, that his duties or powers were clear before him, till they had been thoroughly discussed with the Superintendent in person.

The qualities in which he manifested a high order of genius were especially those by which he governed men. Had his tastes been military or political, instead of scientific, none the less would he have been a Chief. It may safely be said that by no act of his life did he ever curtail any man's means of usefulness, or fail, whenever it was in his power, to render available whatever abilities might be disclosed. Justice and even-handed firmness controlled his action. No man was ever readier to acknowledge and atone for a wrong done by him, in thought or deed. Cautious in plan, bold in action, generous without impetuosity, as courteous and considerate of his subordinates as though they had been his superiors, ever as open to conviction as to argument, such was his noble character. With these traits was united a feminine tenderness of heart, and an intensely sympathetic nature. To him all came for comfort in personal sorrow, for sympathy in bereavement, for help in calamity. And his purse, like his heart, stood open to his friends, and to the needy.

In a critical analysis of his character, the traits which I have just mentioned would occupy the most prominent place, but with them the mention of one other quality would be imperative. I allude to his keen appreciation of humor, his love of pleasantry and jest, and his social geniality. These alleviated the cares of severe administrative duty, and the anxieties inseparable from his official position,—peculiarly great in our own form of government, where the needful appropriations are made by legislative bodies continually varying, and where the personal hostility of a single individual may be made the occasion for denunciation of a national work. Without his social qualities, and his power of banishing at will the oppression of grave cares, his precious life would scarcely have been spared one-half so long, as it was in fact vouchsafed us. This love of humor, so essential in a well balanced mind, was his in no stinted degree, and often was he the center of a group of delighted friends, whose enjoyment of his fun was scarcely exceeded by their reverence for his intellectual and moral worth.

His administration of the Coast-Survey was by no means an easy one in its political relations. For many years there was scarcely a session of Congress, without some vehement attack upon the Survey in each House, made for the purpose of defeating the appropriations. The causes of these were various, and many of them will occur to you without enumeration. Hostilities growing out of his original appointment and incapable of being allayed, jealousies on the part of other institutions professedly established for kindred purposes, resentments on the part of persons subjected to discipline, furnished fuel for the flame, in the form of ready assistance to whatever threatened the welfare of the Survey, come from what quarter it might. At one period, the representatives from inland districts, impelled by narrow local jealousy, opposed the expenditure of public money for purposes which they supposed not so useful to their own States as to others. At another time certain naval officers disseminated, to a considerable extent, the impression that, because it was a Coast which was the subject of Survey, the work should be placed in charge of nautical men. At still another date, similar claims were urged in behalf of the

army. But the minds of the people, and of their legislators became enlightened, so that for some time before the recent war, the facts were recognized, that the services rendered were to no particular State so much as to the nation, and that the helpful aid of army and of navy were alike requisite, as well as that of a corps of civilians trained exclusively to the work.

One of the most violent of these organized attacks was made about the year 1849, and doubtless resulted in permanent good to the Survey, from the multitude and earnestness of the manifestations which it called forth from all parts of the United States, and from Europe. The learned Societies, the Insurance offices, Chambers of Commerce and Boards of Trade, throughout the land, hastened to send their testimony to the services which it was rendering, and their earnest protests against any interference. Men of the highest eminence throughout Europe joined in the appeal. In August, 1849, Schumacher wrote him:

"In an epoch like this, where everywhere associations are formed for material interests, it is high time that we also unite our forces, to protect the higher and purer scientific interests. . . . You will see by the inclosed letters of our common friends, Mr. Arago and Baron Humboldt, how anxious they are to know if the great work you have undertaken will remain in the hands to which the whole scientific world would have intrusted it. Let us hope that the fear they entertain is only founded upon vague rumors. Your country must be proud to call you her own, and will repay you in gratitude what you do for her scientific glory."

A month previous Arago had written to their common friend:

"There is a rumor here that Mr. Bache is threatened with losing the place he is filling in so distinguished a manner. I beg of you to give me information about it, so that we can join with all the most distinguished scientists of Europe to prevent so great a misfortune. The officers of the American navy are proverbial for their gallantry; they know perfectly well every part of the nautical art; still, I shall do them no wrong by stating that Mr. Bache stands higher than they by a hundred cubits in matters of Geodesy, and Physics of the Globe. And I will prove by numerous examples how very useful it is even for the truth of the results, that a man should be superior to the task intrusted to him."

So also Humboldt:

"You know better than I do in how high an estimation the Direc-

tor of the Survey of the Coast stands, not only among us, but among all the most illustrious men, who in France and England are interested in the study of Geography and Nautical Astronomy. . . . In a region of the globe where the direction of oceanic currents, the difference of temperature produced by these currents and by the upheaval of the bottom, and the direction of the magnetic curves offer so important phenomena to navigators,—such a great work could not be better placed than in the hands of Dr. Bache. The government of the United States has acquired a new right to our gratitude by protecting nobly this that has already fixed the attention of the Hydrographers and Astronomers of Europe. I should be glad to think that in a country, where I am honored with so much regard, my feeble testimony might contribute to enliven the interest which is due to the excellent labors of Dr. Bache."

The President of the Royal Geographical Society of London, in his Annual Address to the Society in 1852, commented with expressions of strong admiration upon the practical management of the Survey, and characterized it as "one of the most perfect exemplifications of applied science of modern times." And six years later, Murchison, then President of the same Society, in his address presenting to him the great gold medal, used this language:

"Whether we regard the science, skill and zeal of the operators, the perfection of their instruments, the able manner in which the Superintendent has enlisted all modern improvements into his service, the care taken to have the observations accurately registered, his modest and unpretending demeanor, or the noble liberality of the Government, tempered with prudent economy, all unprejudiced persons must agree that the trigonometrical Survey of the United States of America stands without a superior."

Such expressions as these, uttered from all quarters at home and abroad whenever the work was menaced, speedily removed in every instance, all occasion for alarm to its friends.

The last serious attack was in 1858, when Bache's firm and unyielding defence of the scientific interests at stake in the well-known contest at the Dudley Observatory, was the occasion for the most virulent of all the onslaughts ever made upon him. At first it was supposed that by the inauguration of a regular system of attack upon the Survey, Bache would be frightened from his position at the Observatory. But where either moral principles or the interests of science were involved

he knew no fear, and what was commenced as a means of alarm, was continued only in the spirit of revenge. The daily press was filled with attacks upon the Survey, its policy, its value and its Superintendent, some of them violent and some insidious. Pamphlets were prepared and sent to members of Congress at their homes, and to all prominent public men in Washington. Relations were opened with former opponents of the Survey, and with all who could be found, supposed to be disaffected towards it in any way. It is a source of peculiar regret that obvious circumstances preclude me from entering into many particulars and illustrations here, which others might have laid before you; and which would place in strong relief the magnanimity, the force, and the lofty chivalry of spirit in our lost leader. He stood like a rock, upon which the storm beat in vain.

> "O iron nerve, to true occasion true!
> O fallen, at length, that tower of strength
> Which stood four-square to all the winds that blew!"

It will forever be among my most precious memories that in that time of trial, it was my privilege to receive his approval from the beginning. The very last, the passive abandonment of the moral struggle, when physical force became involved, was the only step which failed of his full indorsement. This was to me the greatest pain of all; but happily dispersed, when upon farther knowledge of the circumstances, his opinion was changed, and his priceless approval extended to the whole of my course in that bitter contest.

A year later, at the Springfield meeting of this Association, a Report on the History and Progress of the American Coast-Survey, was presented by a committee of twenty men of science not connected with the Survey, and who had been appointed for the purpose at a previous meeting. Fortunately the attacks of the preceding year had failed of their effect, in consequence of the manifestly personal motives which inspired them, and of their evident connection with the Observatory at Albany. This able report, commenced by Judge Kane of Philadelphia, and after his death prepared by our honored late President, Dr. Barnard, then at the head of the University of Mississippi, presents a thorough analysis of the great work ac-

complished, and the strong language in its praise, with which the report concludes, received the unanimous indorsement of the Committee.

But this inadequate sketch of Bache's services would be yet more incomplete, were the record of his scientific achievements, during the last twenty years of his life, confined to the work officially done through the Coast-Survey, which he had made so essential to science in America.

His own investigations and discoveries during this period have not been enumerated, and though from the nature of the case they were mostly derived from data collected by the operations of the institution to which he devoted his energies, none the less were they the fruits of his own intellectual labor. I have endeavored to prepare a catalogue of his own memoirs,—a large proportion of which were presented to this Association, which he fondly cherished, and upon the sessions of which he was an unfailing attendant.

Most of these memoirs are upon subjects connected with the new methods which he introduced into the Survey, the measurement of bases, or the phenomena and laws of the tides, of the Gulf-stream, and of terrestrial magnetism. These phenomena and laws he unraveled and developed, to an extent which warrants us in regarding him as the principal source of our knowledge of their more complicated action. To our Providence meeting, in 1855, he communicated a remarkable paper, on the progress of an earthquake-wave in December previous, which was detected on our Pacific coast by means of the self-registering tide-gauges. This wave he traced from Japan,—where it had occasioned serious damage to the Russian frigate "Diana," in the port of Simoda, and where the highest of five successive waves was about thirty feet,—to Peel's Island, one of the Bonin group, where the first wave rose to an altitude of fifteen feet above the high-water level,—and thence to the shores of Oregon and California, where the disturbance was recorded at Astoria, San Francisco and San Diego; at which last-named station eight waves were traced upon the tide-gauge, the largest being six inches in height. From these observations he deduced the speed of six miles a minute as the rate of the wave's motion, and the mean depth of the Pacific as

about 2300 fathoms on the San Francisco path, and about 2100 fathoms on the San Diego path.

Meanwhile he had conducted, in connection with General Totten and Admiral Davis, special researches of a highly important character, nominally independent of his position as Superintendent of the Coast-Survey. He thus served as Commissioner upon the harbors of New York, Boston, Charleston, Portland, and the Cape Fear River; and the reports contain investigations concerning the formation of obstructions in rivers and harbors, and the laws controlling the improvement and deterioration of channels, which have largely contributed to our knowledge of these laws.

At the Albany meeting of this Association, in 1856, he communicated among other valuable papers, a very important memoir from himself and Mr. Hilgard jointly, upon the general distribution of Terrestrial Magnetism in the United States. At Montreal, he described the chronometric longitude expedition, then recently carried out by him in person; the base-line which he had just measured upon Epping Plains in Maine; and the principal characteristics of the winds of the Western Coast of the United States. At Baltimore he presented an investigation of the personal equation in determining latitudes by Talcott's method; and a memoir on the tidal currents of New York Bay. At Springfield he gave a discussion of results deduced from the declinometer-observations made at Girard College, from fifteen to twenty years before; and a third memoir upon the Gulf-stream, showing the distribution of temperature in the waters of the Florida channel and straits. At Newport he added two papers containing theoretical results from the magnetic observations at Girard College, one upon the solar-diurnal variation and the other upon the influence of the moon upon the needle.

When the outbreak of the rebellion gave a new direction to the energies of all the Bureaus of the government, Bache was of course found at his post, equal to the occasion. When a secret commission of three was organized to devise measures and provide plans for the blockade of ports, or the capture of important points on the sea-board of the insurrectionary territory, the military and naval skill of officers high in command

in their respective services was reinforced by the knowledge and sagacity of Bache, whose personal ability gave added value to the charts of the Survey. More than one General-in-chief looked to him for daily counsel during those days of doubt and anxiety. He carried on experiments for improving the system of signals, he arranged and carried out a multitude of military surveys, and contributed from the Coast-Survey office a large proportion of all the campaign maps furnished to commanding officers. As Vice President of the Sanitary Commission from the first, his leisure from official duties was given to patriotic and humane efforts in behalf of our soldiers. And finally, in 1863, when his native city was threatened with invasion, he offered his personal services for arranging and superintending a system of fortifications. To his intense labor here, under many difficulties and amid numerous other cares, is to be attributed the fatal malady which interrupted his work, and took him from us in the very height of his usefulness, his power and his renown. In the language of the Faculty of his own University,

> "He is justly to be regarded as a martyr to the cause of good government and the principles of human liberty, his death being directly caused by the overtasking of his faculties, in his active and never ceasing endeavors to sustain the authorities of his country against the rebellion, and to promote the efficiency and comfort of those who were fighting in our behalf."

In the spring of 1864 came the blow. It came too in the form which he had himself dreaded; as it had come to Walker before him, as it came to Faraday with him, and as the activity of nervous and mental energy in our own country especially invites it to so many of our intellectual workers. The sword wore out the sheath; the restlessly toiling brain gave way, and refused longer to obey the untiring mind. The repose, then imperative, came too late. After a summer of quiet in his camp, where he nominally continued his personal share in the triangulation which connected the New England stations with Hassler's stations in New York, he sailed for Europe in the autumn, hoping for a benefit from change of scene. A multitude of friends assembled to join in the farewell. The steamer was under the command of Captain Anderson,

himself a scientist of no mean attainments, whose subsequent efficient services in connection with the oceanic telegraph, have deservedly given him a wide reputation, and whose sympathetic kindness alleviated the discomforts of the sea voyage. With touching earnestness, Mr. Bache insisted on carrying with him a set of instruments for determining magnetic constants at points to be visited during his journey. In England, France and Germany he revisited some of his scientific friends of old, but the excitement proved more than he could well bear, and this pleasure it subsequently became necessary for him to forego. It was a source of special gratification to him to meet Struve at Rome, where he was temporarily sojourning. After an absence of eighteen months in Europe he returned, and even then his friends could not utterly surrender their hopefulness. But the fatal blow had fallen, and time only rendered its effects more manifest. At Newport, Rhode Island, on the 17th of February, 1867, his spirit cast aside the broken instrument which it could no longer govern, and returned free and rejoicing, unto God who gave it.

I will not undertake to enumerate the manifestations of honor, which were paid the lifeless clay, as it was carried from Newport to its last resting place in Washington. In New York and Philadelphia civic honors awaited it, and it reposed in state while the tokens of respect and gratitude were paid. Amid emblems of mourning, flags waving down the masts and minute guns breaking the silence, it passed along the streets. A solemn and reverent company awaited it at the national capital, and guarded it till it should be no more seen on earth. And when the last rite was over, there was not one of that great assembly who did not feel that he had lost a treasured friend, and that his country's loss was beyond utterance.

Gentlemen, I have far exceeded the limits of time appropriate for an oral address. Yet I have failed to convey more than an outline of the work he did, the services he rendered, the man he was. But my imperfect words are not his eulogy. Far and wide over this great land are scattered memorials of the love and honor of his countrymen. Above the Pacific coast a lofty mountain bears his name, and towers over the shores to which so many of his thoughts were devoted, a fitting

monument to his memory. Amid the polar regions of eternal ice and snow, still his name records the services he rendered to science there; and far in the North-west, the clear expanse of a broad lake repeats the familiar sound.

But I will attempt to recall to you no more.

"Peace! His triumphs will be sung
By some yet unmoulded tongue
Far off in summers that we shall not see. . . . .
Ours the pain, be his the gain! . . .. .
Yet lifted high in heart and hope are we,
When we remember that for one so true
There must be other, nobler work to do.

He is gone who seemed so great!
Gone; but nothing can bereave him
Of the force he made his own
Being here; and we believe him
Something far advanced in state,
And that he wears a truer crown
Than any wreath that man can weave him."

While greatness, conjoined with goodness, is held in reverence by men; while the intellectual advancement of a nation is rewarded by the gratitude of her sons; while the history of American science is read or written;—yes, until winds and tides no longer rise and fall; until shore and sea no longer follow their varied course in beach and cliff, headland and bay; until the sailor no longer fears the sunken reef, the drifting current, or the treacherous rock; until the mystic forces of earth fail to exhibit their varying play, and the lamps of heaven no longer guide the wanderer,—shall reverence, honor and gratitude adorn the name of ALEXANDER DALLAS BACHE.

CORRIGENDUM.—On page 2, line 5 from below, for "only child," read "only surviving child;" since the son, Francis Folger, lived to be five years old.

PUBLISHED WORKS

OF

ALEXANDER DALLAS BACHE.

1829—Feb. On the Specific Heat of the Atoms of Bodies. *Journ. Phila. Acad. Nat. Sci.* vi, 141.

1830—May. On the Inflammation of Phosphorus in a partial vacuum. *Amer. Journ. Sci.* xviii, 372.

1831—April. Safety Apparatus for Steamboats, being a combination of the Fusible Metal Disk with the common Safety-Valve. *Journ. Franklin Inst.* vii, 217; *Amer. Journ. Sci.* xx, 317.

1831—March. Report of the Committee of the Franklin Institute of Pennsylvania appointed May, 1829, to ascertain by experiment the value of Water as a moving Power. *Journ. Franklin Inst.* vii, 145, viii, ix, x, &c.

1831—Oct. Meteorological Observations during the Solar Eclipse of Feb. 12, 1831. *Trans. Amer. Phil. Soc.* iv, 132.

1832—July. Translation of Berzelius's Essay on Chemical Nomenclature. *Amer. Journ. Sci.* xxii, 248; *Philadelphia*, 1832.

1832—July. Notice of Experiments on Electricity developed by Magnetism. *Journ. Franklin Inst.* x, 66; *Amer. Journ. Sci.* xxii, 409.

1832—Oct. Alarm to be applied to the interior flues of Steam Boilers. *Journ. Franklin Inst.* x, 217.

1832—Nov. On the Diurnal Variation of the Magnetic Needle. *Trans. Amer. Phil. Soc.* v, 1.

1833—March. Elementary View of the Application of Analysis to Reflection and Refraction. An Appendix to Sir David Brewster's Treatise on Optics. *Philadelphia*, 1833. pp. 95.

1833—July. Translation of Avogadro's Memoir on the Elastic Force of the Vapor of Mercury. *Amer. Journ. Sci.* xxiv, 286.

1833—July. Note of the Effect upon the Magnetic Needle, of the Aurora Borealis, visible at Philadelphia on the 17th of May, 1833. *Journ. Franklin Inst.* xii, 5; *Amer. Journ. Sci.* xxvii, 113.

1833—Nov. Attempt to fix the date of Dr. Franklin's Observation, in relation to the North-east Storms of the Atlantic States. *Journ. Franklin Inst.* xii, 300.

1833—Dec. Report of Experiments on the Navigation of the Chesapeake and Delaware Canal by Steam. *Journ. Franklin Inst.* xii, 361.

1834—Jan. Observations on the Disturbance in the direction of the Horizontal Needle, during the occurrence of the Aurora of July 10, 1833. *Journ. Franklin Inst.* xiii, 1; *Amer. Journ. Sci.* xxvii, 118.

1834—Jan. Report of Managers of the Franklin Institute, in relation to Weights and Measures. Presented in Compliance with a Resolution of the House of Representatives of the State of Pennsylvania. *Journ. Franklin Inst.* xiv, 6; *Philadelphia*, 1834.

1834—June. Analysis of some of the Coals of Pennsylvania [made jointly with Prof. H. D. Rogers]. *Journ. Phila. Acad. Nat. Sci.* vii, 158.

1834—Oct. On the Variation of the Magnetic Needle. *Amer. Journ. Sci.* xxvii, 385.

1834—Nov. Observations to determine the Magnetic Dip, at Baltimore, Philadelphia, New York, West Point, Providence, Springfield and Albany [made jointly with Prof. E. H. Courtenay]. *Trans. Amer. Phil. Soc.* v, 209.

1834—Nov. Meteoric Observations on and about Nov. 13, 1834. *Amer. Journ. Sci.* xxvii, 335; *Journ. Franklin Inst.* xvi, 369.

1835—Jan. Note relating to the Hardening of Lime under Water, by the action of carbonate of potassa, &c., and to the Hardening of Carbonate of Lime in the air, by potassa and soda. *Journ. Frank. Inst.* xv, 6.

1835—March. Meteorological Observations made during the Solar Eclipse of Nov. 30, 1834. *Trans. Amer. Phil. Soc.*, v, 237.

1835—May. Experimental Illustrations of the Radiating and Absorbing Powers, of Surfaces for Heat, of the effects of Transparent Screens, of the conducting power of Solids, &c. *Journ. Franklin Inst.* xv, 303; *Amer. Journ. Sci.* xxviii, 320.

1835—May. Replies to a Circular in relation to the occurrence of an unusual Meteoric Display on the 13th of Nov., addressed by the Secretary of War to the military posts of the United States, with other facts relating to the same question. *Amer. Journ. Sci.* xxviii, 305; *Journ. Franklin Inst.*, xvi, 149.

1835—June. Experiments on the efficacy of Perkins's Steam Boilers or Circulators. *Journ. Franklin Inst.* xv, 379.

1835—July. On the comparative Corrosion of Iron, Copper, Zinc, &c., by a saturated solution of common salt. *Journ. Franklin Inst.* xvi, 2.

1835—Nov. Inquiry in relation to the alleged Influence of Color on the Radiation of non-luminous Heat. *Journ. Franklin Inst.* xvi, 289; *Amer. Journ. Sci.* xxx, 16.

1835—Dec. Historical Notice of a Hypothesis to explain the greater quantity of Rain which falls on the surface of the ground than above it. *Journ. Franklin Inst.* xvii, 106.

1836—Jan. Observations upon the facts recently presented by Prof. Olmsted in relation to Meteors seen on the 13th of Nov., 1834. *Journ. Franklin Inst.* xvii, 33; *Amer. Journ. Sci.* xxix, 383.

1836—Jan. Historical Note on the Discovery of the Non-conducting Power of Ice. *Journ. Franklin Inst.* xvii, 182.

1836—Jan. Report of Experiments made by the Committee of the Franklin Institute of Pennsylvania on the Explosions of Steam Boilers, at the request of the Treasury Department of the United States. *Journ. Franklin Inst.* xvii, 1, 73, 145, 217, 289.

1836—Feb. Remarks on a Method, proposed by Dr. Thomson, for determining the Proportions of Potassa and Soda in a mixture of the two Alkalies; with the application of a similar investigation to a different method of analysis. *Journ. Franklin Inst.* xvii, 305.

1836—April. Notes and Diagrams illustrative of the Directions of the Forces acting at and near the Surface of the Earth, in different parts of the Brunswick Tornado of June 19, 1835. *Trans. Amer. Phil. Soc.* v, 407.

1836—May. On the Relative Horizontal Intensities of Terrestrial Magnetism at several places in the United States, with the investigations of Corrections for Temperature, and Comparisons of the Methods of Oscillation in Full and in Rarefied Air [jointly with Prof. E. H. Courtenay]. *Trans. Amer. Phil. Soc.* v, 427.

1836—July. Proposed Forms of Diagrams for exhibiting to the eye the results of a register of the Direction of the Wind. *Journ. Franklin Inst.* xviii, 22.

1837—May. Corresponding Magnetic Observations, in connection with Prof. Lloyd of Dublin, to determine the relative magnetic intensity in Philadelphia, Dublin and Edinburgh. *Proc. R. Irish Acad.* i, 71.

1838—Aug. Note on the effect of deflected Currents of Air on the Quantity of Rain collected by a Rain-gauge. *Rep. Brit. Assoc. Adv. Sci.*, 1838, ii, 25.

1839—May. Report on Education in Europe, to the Trustees of the Girard College for Orphans. 8vo. pp. 666. *Philadelphia*, 1839.

1839—Nov. Comparison of Prof. Loomis's Observations on Magnetic Dip with those obtained by Prof. Courtenay and himself. *Proc. Amer. Phil. Soc.* i, 146.

1839—Nov. Simultaneous Magnetic Observations, made in correspondence with Prof. Lloyd of Dublin. *Proc. R. Irish Acad.* i, 462; *Amer. Journ. Sci.* xli, 212.

1840—March. Observations of the Magnetic Intensity at twenty-one stations in Europe. *Trans. Amer. Phil. Soc.* vii, 75; *Proc. Amer. Phil. Soc.* i, 185.

1840—Nov. Determination of the Magnetic Dip at Philadelphia and Baltimore. *Proc. Amer. Phil. Soc.* i, 294.

1840—Dec. On an Instrument for measuring the changes in the Vertical component of the force of Terrestrial Magnetism. *Proc. Amer. Phil. Soc.* i, 311.

1841—May. Diagram of the Direction and Force of the Wind, and amount and rate of Rain-fall during the severe gust of April 2, 1841. *Proc. Amer. Phil. Soc.* ii, 56.

1841—July. On Observations of the Magnetic Dip, made at Baltimore by Mr. Nicollet and Major Graham. *Proc. Amer. Phil. Soc.* ii, 88.

1841—Nov. Account of the Formation of Cumulus Cloud from the action of a fire. *Proc. Amer. Phil. Soc.* ii, 116.

1842—March. Semi-annual Report of the Principal of the High School, and Report to the Controllers of the Public Schools. *Twenty-fourth Annual Report of Controllers of Public Schools of Philadelphia*, pp. 23, 50.

1842—April. On the application of the Self-registering Rain-gauge to registering the Fall of Snow. *Proc. Amer. Phil. Soc.* ii, 164.

1842—July. Report of the Principal of the Central High School for the year ending July, 1842. 8vo. pp. 120. *Philadelphia.*

1842—Oct. Address delivered at the close of the Twelfth Exhibition of American Manufactures, held by the Franklin Institute.

1842—Dec. On a modification of Lloyd's induction Inclinometer. *Proc. Amer. Phil. Soc.* ii, 237.

1843—Jan. On a new Dew-point Hygrometer. *Proc. Amer. Phil. Soc.* ii, 249.

1843—May. Results of two years Observations of the Magnetic Elements, and of the Temperature, Pressure and Moisture of the Atmosphere at the magnetic observatory of Girard College. *Proc. Amer. Phil. Soc.* iii, 90.

1843—May. Account of an Instrument for determining the Conducting Power of bodies for heat. *Proc. Amer. Phil. Soc.* iii, 132.

1843—May. Account of Observations at Philadelphia and Toronto during the Magnetic Disturbance of May 6, 1843, and their bearing upon the question of the kind of instruments and observations, appropriate to determine such phenomena. *Proc. Amer. Phil. Soc.* iii, 175.

1845—Feb. Report to the Treasury Department on the progress of construction of Standard Weights and Measures. *Senate Doc.* 149, *28th Congress, 2d Session.*

1847—Dec. Description of a new Base Apparatus used in the United States Coast Survey. *Proc. Amer. Phil. Soc.* iv, 368.

1848—Dec. On a new Method of observing Transits. *Monthly Not. R. Astr. Soc.* ix, 123; *Bull. Acad. Sci. Brussels*, xvi, 313; *Astr. Nachr.* xxviii, 273.

1849—Aug. Comparison of the Results obtained in Geodesy by the application of the theory of Least Squares. *Proc. Amer. Assoc. Adv. Sci. Cambridge*, 1849, p. 102.

1849—Aug. On the progress of the Survey of the Coast of the United States. *Proc. Amer. Assoc. Cambridge*, 1849, p. 162.

1850—March. Notes on the results of Observations of the Direction and Force of the Wind at the Coast-Survey stations at Mobile Point and at Cat Island, Gulf of Mexico. *Proc. Amer. Assoc. Charleston*, 1850, p. 50.

1850—March. Abstract of a communication on the recent Progress of the Telegraphic Operations of the United States Coast-Survey. *Proc. Amer. Assoc. Charleston*, 1850, p. 122.

1850—Aug. Method used in the Coast-Survey for showing the results of Current Observations. *Proc. Amer. Assoc. New Haven*, 1850, p. 70; *C. S. Rep.*, 1850, p. 136.

1850—Aug. Remarks upon the Meeting of the American Association at Charleston, in March, 1850. *Proc. Amer. Assoc. New Haven*, 1850, p. 159.

1850—Aug. Notes of a discussion of Tidal Observations, in connection with the Coast-Survey, made at Cat Island in the Gulf of Mexico. *Proc. Amer. Assoc. New Haven*, 1850, pp. 281; *Amer. Journ. Sci.* xii, 341; *C. S. Rep.*, 1851, p. 127.

1851—May. Current Chart of New York Bay, from Observations in the Coast-Survey. *Proc. Amer. Assoc. Cincinnati*, 1851, p. 43.

1851—May. Comparison of curves showing the Hourly Changes of Magnetic Declination at Philadelphia, Toronto and Hobarton from April to August, and from October to February, and for March and September. *Proc. Amer. Assoc. Cincinnati*, 1851, p. 62.

1851—May. On the determination of the Velocity of Sound by the Method of Coincidences. *Proc. Amer. Assoc. Cincinnati*, 1851, p. 75.

1851—May. Notes on the use of the Zenith Telescope in determining Latitudes in the Coast-Survey by Talcott's method, and on the reduction of the observations. *Proc. Amer. Assoc. Cincinnati*, 1851, p. 151; *Amer. Journ. Sci.* xiv, 191.

1851—Aug. Additional notes of a discussion of Tidal Observations made in connection with the Coast-Survey at Cat Island, Louisiana. *Proc. Amer. Assoc. Albany*, 1851, p. 94; *Amer. Journ. Sci.* xiv, 346; *C. S. Rep.*, 1852, p. 111.

1851—Aug. Notes on the Tides at Sand Key, near Key West, Florida. *Proc. Amer. Assoc. Albany*, 1851, p. 138.

1851—Aug. Address on retiring from the duties of President of the American Association for the Advancement of Science. *Proc. Amer. Assoc. Albany*, 1851, p. 41.

1852—March. Report on the Harbor of Charleston, S. C. [As Chairman of a Committee.]

1853—July. On the Tides at Key West, Florida, from observations made in connection with the United States Coast-Survey. *Proc. Amer. Assoc. Cleveland*, 1853, p. 32; *Amer. Journ. Sci.* xviii, 305; *C. S. Rep.*, 1853, p. 71.

1853—July. On the Tides of the Western Coast of the United States, from observations at San Francisco, California, in connection with the United States Coast-Survey. *Proc. Amer. Assoc. Cleveland*, 1853, p. 42; *Amer. Journ. Sci.* xxi, 1; *C. S. Rep.*, 1853, p. 77.

1854—May. Preliminary Determination of Co-tidal Lines on the Atlantic Coast of the United States, from the Coast-Survey tidal observations. *Proc. Amer. Assoc. Washington*, 1854, p. 107; *Amer. Journ. Sci.* xxi, 14; *C. S. Rep.*, 1854, p. 147.

1854—May. Comparison of the Diurnal Inequality of the tides at San Diego, San Francisco and Astoria, on the Pacific Coast of the United States, from observations in connection with the Coast-Survey. *Proc. Amer. Assoc. Washington*, 1854, p. 161; *Amer. Journ. Sci.* xxi, 10; *C. S. Rep.*, 1854, p. 152.

1854—May. On the Distribution of Temperature in and near the Gulf-stream, off the Coast of the United States, from observations made in the Coast-Survey. *Proc. Amer. Assoc. Washington*, 1854, p. 134; *Amer. Journ. Sci.* xxi, 29; *C. S. Rep.*, 1854, p. 156.

1854—June. Second Report on the Harbor of Charleston, S. C.

1855—Feb. Discussion of Observations for the Isodynamic, Isogonic, and Isoclinal Curves of Terrestrial Magnetism, on and near the line of the Boundary Survey between the United States and Mexico, made in 1849–1852, under the order of W. H. Emory, and combined with observations at San Francisco, California, and Dollar Point (E. Base), and Jupiter, Texas. *Mem. Amer. Acad. Arts and Sci.* v, 372.

1855—Aug. Approximate Co-tidal lines of the Pacific Coast of the United States, from observations in the United States Coast-Survey. *Proc. Amer. Assoc. Providence*, 1855, p. 144; *C. S. Rep.*, 1855, p. 338.

1855—Aug. Notice of the Tidal Observations made on the Coast of the United States, on the Gulf of Mexico, with Type-curves at the several stations, and their decomposition into the curves of Diurnal and Semidiurnal tides. *Proc. Amer. Assoc. Providence*, 1855, p. 152; *Amer. Journ. Sci.* xxi, 128: *C. S. Rep.*, 1855, p. 346.

1855—Aug. Notice of Earthquake Waves on the Western Coast of the United States on the 23d and 25th of Dec., 1854. *Proc. Amer. Assoc. Providence*, 1855, p. 153; *Amer. Journ. Sci.* xxi, 37; *C. S. Rep.*, 1855, p. 342.

1855—Dec. Report to New York Harbor Commissioners, on Surveys of New York Harbor.

1856—Jan. Annual Address before the American Geographical Society, being a paper upon the Gulf-stream. *Bulletin Amer. Geogr. Soc.* ii, 94.

1856—Aug. Approximate Co-tidal Lines of diurnal and semidiurnal Tides of the Coast of the United States on the Gulf of Mexico. *Proc. Amer. Assoc. Albany*, 1856, p. 168; *Amer. Journ. Sci.* xxiii, 12; *C. S. Rep.*, 1856, p. 252.

1856—Aug. On the general Distribution of Terrestrial Magnetism in the United States from observations made in the United States Coast-Survey, and others. [In connection with Mr. J. E. Hilgard.] *Proc. Amer. Assoc. Albany*, 1856, p. 187; *Amer. Journ. Sci.* xxvi, 1; *C. S. Rep.*, 1856, p. 209.

1856—Aug. Observations to determine the Cause of the Increase of Sandy Hook, made by the Coast-Survey for the Commissioners on Harbor Encroachments. *Proc. Amer. Assoc. Albany*, 1856, p. 171; *Amer. Journ. Sci.* xxiii, 16; *C. S. Rep.*, 1856, p. 263.

1856—Aug. Notice of observations to determine the Progress of the Tidal Wave of the Hudson River, made by the Coast-Survey for the Commissioners on Harbor Encroachments. *Proc. Amer. Assoc. Albany*, 173; *Amer. Journ. Sci.* xxiii, 17.

1856—Aug. Notes on the progress made in the Coast-Survey in Prediction-tables for the tides of the U. S. Coast. *Proc. Amer. Assoc. Albany*, 1856, p. 166; *C. S. Rep.*, 1856, p. 249.

1856—Oct. Anniversary Address before the American Institute of the city of New York, during the Twenty-eighth Annual Fair. *New York*, 1857, pp. 60.

1856—Dec. Report to New York Harbor Commissioners on the Progress of the Survey of New York Harbor.

1856—Dec. Notes on the Winds of the Coast of the United States on the Gulf of Mexico; *C. S. Rep.*, p. 272.

1856—Dec. Report of Superintendent of Weights and Measures.

1857—Aug. Notice of the determination of the Longitude of Fernandina, Amelia Island, Florida, by means of Chronometer exchanges from Savannah, Georgia. [In connection with Mr. C. A. Schott.] *Proc. Amer. Assoc. Montreal*, 1857, p. 166; *C. S. Rep.*, 1857, p. 314.

1857—Aug. On the Heights of the Tides of the Atlantic Coast of the United States from observations in the Coast-Survey. *Proc. Amer. Assoc. Montreal*, 1857, p. 175; *Amer. Journ. Sci.* xxv, 47; *C. S. Rep.*, p. 342.

1857—Aug. On the Winds of the Western Coast of the United States, from observations in connection with the United States Coast-Survey. *Proc. Amer. Assoc. Montreal*, 1857, p. 183; *Amer. Journ. Sci.* xxv, 52; *C. S. Rep.*, 1857, p. 354.

1857—Aug. Notes on the Measurement of the Base of the primary triangulation of the Eastern section of the Coast of the United States, on Epping Plains, Maine. *Proc. Amer. Assoc. Montreal*, 1857, p. 160; *Amer. Journ. Sci.* xxv, p 58; *C. S. Rep.*, 1857, p. 302.

1857—Dec. Report relative to the New York Harbor Survey. *U. S. Senate Doc* 1858, No. 16.

1858—May. On a supposed Personal Equation in the use of the Zenith Telescope, for determining Latitudes by Talcott's method. *Proc. Amer. Assoc. Baltimore*, 1858, p. 14; *C. S. Rep.*, 1858, p. 184.

1858—May. On the Tidal Currents of New York Bay, near Sandy Hook. *Proc. Amer. Assoc. Baltimore*, 1858, p. 80. *Amer. Journ. Sci.* xxvi, 334; *C. S. Rep.*, 1858, p. 197.

1858—July. Defence of Dr. Gould, by the Scientific Council of the Dudley Observatory. [In connection with Professors Joseph Henry, and Benj. Peirce]. *Albany*, 1858, pp. 91.

1859—Feb. Address before the Alumni Association of the Central High School. *Philadelphia*, 1859.

1859—June. Address in Tribute to the Memory of Humboldt. *The Pulpit and Rostrum*, No. 6.

1859—Aug. General account of the results of the Discussion of the Declinometer observations, made at Girard College, Philadelphia, between the years 1840 to 1845, with special reference to the Eleven Year Period. *Proc. Amer. Assoc. Springfield*, 1859, p. 248.

1859—Aug. Gulf-stream Explorations. 3d Memoir. Distribution of Temperature in the Water of the Florida Channel and Straits. *Proc. Amer. Assoc. Springfield*, 1859, p. 257; *Amer. Journ. Sci.* xxix, 199; *C. S. Rep.*, 1859, p. 306.

1859—Nov. Discussion of the Magnetic and Meteorological Observations made at the Girard College Observatory, Philadelphia, in 1840 to 1845, Part I. Investigation of the Eleven Year Period in amplitude of the Solar Diurnal Variation, and of the Disturbances of the Magnetic Declination. pp. 22. *Smiths. Contrib. to Knowledge*, vol xi, art. iv; *C. S. Rep.*, 1859, p. 278.

1860—Aug. Lecture on the Gulf-stream. *Proc. Amer. Assoc. Newport*, 1860, p. xlix; *Amer. Journ. Sci.* xxx, 313; *C. S. Rep.*, 1860, p. 165.

1860—Aug. General account of the results of Part II, of the Discussion of the Declinometer observations made at the Girard College, Philadelphia, between 1840 and 1845, with special reference to the Solar Diurnal Variation and its Annual Inequality. *Proc. Amer. Assoc. Newport*, 1860, p. 74; *Amer. Journ. Sci*, xxxi, 197.

1860—Aug. Abstract of a discussion of the Influence of the Moon on the Declination of the Magnetic Needle from the observations at the Girard College. *Proc. Amer. Assoc. Newport*, 1860, p. 83; *Amer. Journ. Sci.* xxxi, 98.

1860—Sept. Observations of the Solar Eclipse of 1860, July 18, at the Coast-Survey Station, Gunstock mountain. *Astron. Journ.* vi, 157.

1862—June. Discussion of the Magnetic and Meteorological Observations, made at the Girard College Observatory, Philadelphia, in 1840 to 1845. Part II. Investigation of the Solar Diurnal Variation of the Magnetic Declination and its Annual Inequality. pp. 28. *Smiths. Contrib. to Knowl.*, vol. xiii, art. v; *C. S. Rep.*, 1860, p. 293.

1862—June. Discussion of the Magnetic and Meteorological Observations, made at the Girard College Observatory, Philadelphia, in 1840 to 1845. Part III. Investigation of the Influence of the Moon on the Magnetic Declination, pp. 16. *Smiths. Contrib. to Knowl.*, vol. xiii, art. vi; *C. S. Rep.*, 1860, p. 312.

1862—Sept. Abstract of a discussion of the Horizontal Component of the Magnetic Force, from observations made at the Girard College Observatory, Philadelphia, in the years 1840 to 1845. *Amer. Journ. Sci.* xxxiv, 261.

1862—Nov. Discussion of the Magnetic and Meteorological observations, made at the Girard College Observatory, Philadelphia, in 1840 to 1845. Second section comprising Parts IV, V, VI. Horizontal Force. Investigation of the Ten or Eleven Year Period, and of the Disturbances of the Horizontal Component of the Magnetic Force; Investigations of the Solar Diurnal Variation and of the Annual Inequality of the Horizontal Force; and the Lunar effect on the same. pp. 78. *Smiths. Contrib. to Knowl.*, vol. xiii, art. viii; *C. S. Rep.*, 1862, p. 161.

1862—Nov. Abstract of an Investigation of the Solar Diurnal Variation of the Annual Inequality of the Horizontal component of the Magnetic Force, from observations made at the Girard College Observatory, between 1840 and 1845. *Amer. Journ. Sci.* xxxiv, 373.

1862—Nov. Abstract of a Discussion of the Influence of the Moon on the Horizontal Magnetic Force, from observations made at the Girard College Observatory, in the years 1840 to 1845. *Amer. Journ. Sci.* xxxiv, 381.

1862—Dec. Additional Researches on the Co-tidal Lines of the Gulf of Mexico. *C. S. Rep.*, 1862, p. 126.

1863—Jan. Eulogy on Hon. James Alfred Pearce. *Annual Report of Regents Smithsonian Institution for* 1862, p. 100.

1863—May. Abstract of results of a Magnetic Survey of Pennsylvania and parts of adjacent States in 1840 and 1841, with some additional results of 1843 and 1862, and a map. *Amer. Journ. Sci.* xxxv, 359; *C. S. Rep.*, 1862, p. 212.

1863—Oct. Records and Results of a Magnetic Survey of Pennsylvania and parts of adjacent States in 1840 and 1841, with some additional Records and Results of 1834, 1835, 1843 and 1862, and a map. pp. 88. *Smiths. Contrib. to Knowl.*, vol. xiii, art. viii.

1864—May. Discussion of the Magnetic and Meteorological observations made at the Girard College Observatory, Philadelphia, in 1840 to 1845. Third section, comprising Parts VII, VIII, and IX. Vertical Force. Investigation of the Eleven (or Ten) Year Period, and of the Disturbances of the Vertical Component of the Magnetic Force, and Appendix on the Magnetic effect of the Aurora Borealis; with an Investigation of the Solar Diurnal Variation, and of the Annual Inequality of the Vertical Force, and of the Lunar effect on the Vertical Force, the Inclination and Total Force. pp. 72. *Smiths. Contrib. to Knowl.*, vol. xiv, art. ii; *C. S. Rep.*, 1863, p. 156.

1865—Jan. Discussion of the Magnetic and Meteorological observations made at the Girard College Observatory, Philadelphia, in 1840 to 1845. Fourth section, comprising Parts X, XI, and XII. Dip and Total Force. Analysis of the Disturbances of the Dip and Total Force; Discussion of the Solar Diurnal Variation and Annual Inequality of the Dip and Total Force; and Discussion of the absolute Dip, with the Final Values for Declination, Dip and Force between 1841 and 1845. pp. 44. *Smiths. Contrib. to Knowl.*, vol. xiv, art. iii; *C. S. Rep.*, 1864, p. 183.

1844 to 1863. Annual Reports of the Progress of the United States Coast-Survey.

1844 to 1848. Annual Reports of Superintendent of Weights and Measures.

1855 to 1863. Tide Tables for the use of navigators, prepared from the Coast-Survey observations, annually.

Also, the following Reports concerning Harbors, jointly with Messrs. Totten and Davis:

1853—March. Report upon Cape Fear River and Harbor.

1854—Oct. Report of Portland Harbor Commission.

1855—March. Second Report of the Commissioners on Portland Harbor.

1855—Dec. Report of Advisory Council of the New York Harbor Commission. *New York Assembly Doc.*, 1856, No. 8.

1856—July. Report of the Advisory Council of the New York Harbor Commission, recommending certain lines in the East and North Rivers, and in Brooklyn. *New York Senate Doc.*, 1857, No. 40, p. 107.

1856—July. Report of the Advisory Council of the New York Harbor Commission on Gowanus Bay and its Improvements. *New York Senate Doc.*, 1857, No. 40, p. 118.

1856—Sept. Report of the Advisory Council of the New York Harbor Commission, on the Hellgate Passage, regarded as a channel of approach to New York Harbor. *New York Senate Doc.*, 1857, No. 40, p. 137.

1856—Oct. Second General Report of the Advisory Council to the Harbor Commissioners, on Lines in New York Harbor. *New York Senate Doc.*, 1857, No. 40, p. 81.

1856—Dec. Report of the Advisory Council to the Commissioners on Harbor Encroachments of New York, in regard to safe and commodious Anchorages in New York Harbor, for the Purposes of the Quarantine of Vessels. *New York Senate Doc.*, 1857, No. 40, p. 132.

1856—Dec. Report of the Advisory Council to the New York Harbor Commissioners, upon Harlem River and Spuyten Duyvil Creek. *New York Senate Doc.*, 1857, No. 40, p. 151.

1857—Feb. Report of the Advisory Council, of Lines for the East River, for the Shore of Long Island, and the East Shore of Staten Island. *New York Senate Doc.*, 1857, No. 126, p. 9.

1857—March. Report of the Advisory Council to the New York Harbor Commissioners, upon the Comparative Map of New York Bay and Harbor, and its approaches, prepared by the Coast-Survey in March, 1857. *New York Senate Doc.*, 1857, No. 126, p. 12.

1860—March. Preliminary Reports of Commissioners on Boston Harbor. *Boston City Doc.*, 1860, No. 37.

1860—Dec. Second Report of United States Commissioners on the Condition of Boston Harbor. *Boston City Doc.*, 1860, No. 97.

1860—Dec. Special Report of the United States Commissioners on Boston Harbor, on the Relation of Mystic Pond and River to Boston Harbor. *Boston City Doc.*, 1861, No. 12.

1860—Dec. Report of the Advisory Council of the Joint Committee of the Massachusetts Legislature on the subject of a Ship Canal to connect Barnstable Bay and Buzzard's Bay. *Mass. Pub. Doc.*, 1864, No. 41.

1861—Sept. Fourth Report of the United States Commissioners on Boston Harbor. *Boston City Doc.*, 1861, No. 62.

1861—Sept. Fifth Report of the United States Commissioners on Boston Harbor. *Boston City Doc.*, 1861, No. 63.

1863—April. Sixth Report of the United States Commissioners on Boston Harbor. *Boston City Doc.*, 1863, No. 53.

1864—March. Seventh Report of the United States Commissioners on Boston Harbor. *Boston City Doc.*, 1864, No. 33.

1864—March. Eighth Report of the United States Commissioners on Boston Harbor. *Boston City Doc.*, 1864, No. 34.

ADDRESS

OF

FREDERICK A. P. BARNARD, LL. D.

EX-PRESIDENT OF THE ASSOCIATION.

GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—

YOU are assembled to day for the third time since the revival of your association in 1866. On the occasion of that revival it was my privilege to be your presiding officer—a privilege and an honor which I shall always remember with the highest gratification and pride—but of which one of the resulting obligations has never been fulfilled. My unavoidable absence at your last meeting had, as I supposed, been tacitly accepted as a sufficient apology for the failure; but your standing committee, with a fidelity of recollection which I had hardly anticipated, have preserved the record of the debt, and have consequently assigned me the not unpleasing duty of welcoming your return at this time to your annual deliberations.

The city which, on the present occasion, has extended to American science the encouragement of its generous hospitality, is one which has been heard of before in connection with large assemblies professing to represent our country in one or another of its aspects. But this is the first time that a great convention has been assembled here which could claim to be called in the strictest sense national; a convention having no aims likely to engender suspicion in any quarter, and intent on no proceedings liable to be watched by any jealous eyes. For this is the first of the national conventions which the political, social, commercial and intellectual prominence of Chicago has attracted to this spot, of which the declared platform has been entirely catholic and universally acceptable; the

first which could justly hope to enlist the sympathies of all parties, all creeds, all states, all sections alike. It is gratifying to know that there is one subject in regard to which the whole world of mankind have a common interest, one subject on which there can be but one party. Such a subject is that which occupies us. For the object of science is truth. Its progress is the progress of civilization, its encouragement is the encouragement of the arts of life, and the enlargement of the comfort and the happiness of the human race.

Of these truths your presence here to day is a striking illustration. The very possibility of such a general gathering of the votaries of science, is a possibility which science herself has created. You are assembled from every point within the limits of our vast territory. Some of you have left your homes a thousand, some fifteen hundred, miles behind you. Three days ago you were in your laboratories, your lecture halls, your quiet studies, pursuing tranquilly your ordinary occupations, and to day finds you assembled here. The power by which you have been enabled thus to triumph over space is the gift of science to the industrial arts. The iron way which has brought you is her creation; the iron arm which has dragged you, has been clothed by her with its herculean strength. Were engineering science at this time no farther advanced than it was when the great commonwealth of Illinois, but a brief half century ago, first took her place in the sisterhood of the states, the time it would consume, the expense it would involve, and the excessive fatigue it would entail to bring together a body of representative men from points so widely separated as the banks of the St. Lawrence, and the shores of the Gulf of Mexico, would render an assembly like the present a thing wholly impracticable. It is then to science herself that we owe it that we are able, with so little inconvenience, to hold our annual consultations for the advancement of science.

Yet it may be doubted whether this fact has to any great degree occupied your thoughts on your way hither. You have been much less intent upon what science has done than upon what she has yet to do. Accomplished facts have for the investigator but a secondary interest. He is even prone almost to forget them in the earnestness with which he addresses him-

self to the things which remain to be accomplished. There are a thousand truths, and applications of truths, which in the day of their novelty fastened universal attention, but which having become familiar, and having been added to the permanent treasury of knowledge, have at length almost ceased to be remembered as the gifts of science to civilization. Who, for instance, that uses now the electric telegraph, thinks of the slow process of evolution by which, in the hands of Œrsted and Ampère and Arago and Henry, the laws of electro-dynamics were gradually unfolded; or of the happy sagacity by which, in the laboratories of Daniell and Grove and Bunsen, the powerful and persistent electro-motors now in universal use were conceived and perfected; or recognizes the fact that but for the combined results of all these labors, this wonderful instrument of communication could have had no existence? Or who that pauses for a moment before the show window of a print shop in any one of our great cities, to inspect curiously the latest photograph of the pyramids of Egypt, the mountain gorges of California, the glaciers of the polar seas, or the ruins of Mexico or Peru, remembers, if he ever knew, the weary and disappointing toils protracted through years, by which Daguerre and Talbot, and Niepce de St. Victor succeeded at last in permanently fixing the fleeting images of the camera?

These illustrations lead me to remark that, in the history of every new truth brought to light by scientific investigation, there are three stages corresponding to three periods in the life of man. The first may be likened to the period of birth and infancy, deeply interesting to the parent or to those for whom infancy has charms, but regarded with indifference by the world in general; the second answers to the youth of the individual, during which feebleness is ripened into strength, and the helpless child is trained to a useful vocation; and the third is the period of maturity in which the full-grown man becomes merged in the society which surrounds him, where, though his usefulness is recognized, few trouble themselves to inquire beneath what roof he was cradled, or under what tutelage his nascent powers were developed.

In the observation of Volta upon certain curious disturbances of electrical equilibrium, we have an example of a truth in its

infancy: in electro-chemistry and electro-magnetism, in the alkaline metals, in the galvanoplastic arts, the electric telegraph, and the light houses of Dungenness and La Hève, we have the same truth in its vigorous maturity. In the mercurial vacuum produced first by Torricelli, we have another elementary truth. In the engine of Newcomen, destined by successive transformations to pass into the powerful motor which is the soul of modern industry on the land and of modern commerce on the ocean, we have this also in its later phases of development and useful application. Galileo demonstrated the isochronism of the oscillation of suspended bodies; the consequence is before our eyes every day in the pendulum clock. Wollaston marked the curious fact of the transverse striation of the solar spectrum. Bunsen and Kirchhoff converted this into an instrument of chemical research, and Huggins, Miller, Secchi and others have more recently read by its help the constitution of the sun, the fixed stars and the nebulæ. The applications of truths of science to the industrial arts are so numerous, that there is scarcely any article we handle in our daily life, which does not furnish an illustration. And all these things serve to show how matured science passes gradually into common knowledge, while nascent science is always the exclusive possession of the limited class who make its culture a specialty.

This class, though limited, hold to the world in general a position and a relation exceedingly important. They may be likened to the pioneer corps of an army marching through an unexplored and difficult country. They break the path in which the multitude are to follow. Their place is in the front, and their faces are turned toward the wilderness which the invasion has not yet reached. And so our pioneers are always found energetically operating in the border region which divides the known from the unknown; and they too like the others, forgetting or disregarding the things that are behind, are chiefly intent on effecting new encroachments upon territory as yet unconquered. Their task is difficult. Their numbers are few and scattered. They feel, therefore, the need of often drawing near together, and taking counsel of each other, for the promotion of their common interests. It is

thus that they have been led to organize themselves into permanent associations for mutual assistance and encouragement. Such associations have accordingly grown up in every land in which science is cultivated, and their influence has been eminently beneficial in promoting unity and efficiency of action, and in keeping alive the zeal of those who compose them, for the advancement of the common cause. On this side of the Atlantic your association is the most prominent example of this class of organizations.

It is a usage sanctioned by time, that your retiring president, in laying down his office, should improve the opportunity to glance briefly over the general field of science, and present in outline the principal facts of progress which have marked the history of the year that has passed. It is not quite easy, nevertheless, in an area of intellectual activity so extended, and where the laborers are so numerous, to make such a survey exhaustive; and where investigation is so steadily continuous, it is equally difficult to decide how much belongs to the history of a particular year, and to adjust the achievements of science severely to the calendar.

This last difficulty is illustrated in the very first topic to which I shall draw your attention. Since your last meeting the Smithsonian Institution has published the fifteenth volume of its "Contributions to Knowledge," containing the new investigation of the orbit of the planet Neptune, by Professor Simon Newcomb of the United States Naval Observatory. But this able discussion, though only now formally published, already dates back some two or three years, and has been for some time in the hands of astronomers. It has, nevertheless, an interest which will justify a brief mention here. The theory of this planet has long been in a condition so unsatisfactory as to demand thorough revision. And since our country was the first to produce an investigation of the perturbations of the body, and an ephemeris of its movements, in the contributions of Professor Peirce to the Monthly Notices of the Royal Astronomical Society, and in the discussions and tables published by the lamented Walker among the earlier Smithsonian papers, it is gratifying to find that the work of reconstruction has been undertaken and has been so well

performed by an American hand. The necessity of the revision appears in the fact that, in 1863, the ephemeris of Walker was 33″ in error, and that of Kowalski, published some years later, 22″. Before the end of the present century, the error of either of them must become enormous—not less than 5′. The earlier theories were founded on a very limited period of observation; that of Walker on the observations of but little more than a year, during which the planet had moved only 2½°. In the twenty years which have since elapsed, the progress of this body in longitude has exceeded 40°; so that the influence of errors of observation upon the value of the elements is tenfold less than it was when Mr. Walker undertook the investigation. Professor Newcomb has produced a theory which represents the observations since the discovery of the planet in 1846, and also the unconscious observation of the same body by Lalande in 1795, with a maximum error, usually negative, less than 2″ and a mean error less than 1″. He has discussed the question whether the observations can be better satisfied by any changes in the elements of the orbit and in the masses of the disturbing planets, and has concluded that, while no admissible change in the mass of either Jupiter or Saturn will effect the perturbations of Neptune, neither will any correction of that of Uranus do so which is not at once excessive and unreliable. Considering the question whether the remaining errors are owing to an extra-Neptunian planet as yet unknown, he decides against the present evidence of the action of any such body; but regards this negative result as not by any means decisive against the existence of such a planet, because the errors of a slow moving planet require for their developement a much longer time than those of planets of short period, these errors developing themselves not in proportion to the time, but to the square of the arc moved over. When Neptune was detected through the perturbations of Uranus, this latter planet had been observed through an arc of 270°; and the two planets had been in conjunction. Yet notwithstanding these favoring circumstances, Uranus had only departed about 5″ from an elliptic orbit since its first discovery. Had it been observed only through an arc of 120° (something less than half as great) its deviations would not

have exceeded 1″. Whether, therefore, Neptune is the exterior planet of the solar system or not, cannot be at present positively decided, and no evidence to throw light upon the question can be looked for in the anomalies of its movements during the present century.

This valuable contribution to astronomical science suggests another, more recent, by the same hand. From the observations of the planet Mars in opposition made in 1862, at Washington, Albany, Greenwich, Leyden, Helsingfors and Pulkova, in the northern hemisphere, and at the cape of Good Hope and Santiago de Chili in the southern, Mr. Newcomb has deduced a new value for the solar parallax, which he makes to amount to 8″.855, with a probable error of 0″.02. The value of this element is perhaps the most important of all constants in physical astronomy; since on it depends our knowledge of the distance of the sun, which is the unit of measurement in the celestial spaces. The value heretofore received, 8″.58, deduced by Encke, from the observations of the transit of Venus made in 1769, has always been regarded with a certain distrust, founded on circumstances not wholly satisfactory attending the Norwegian observations. Mr. Leverrier had greatly strengthened these suspicions, by discussing the observations of the sun, in reference to the lunar inequality of its longitude, which led him to the conclusion that the parallax should be increased to 8″.95. He had arrived at a similar result from a careful revision of the theories of the planets Venus and Mars, according to which it appeared that such an increase of the parallax was necessary to satisfy the observations of those bodies.

In the mean time Mr. Fizeau attempted his ingenious determination of the velocity of light, by observations made at limited distances on the earth's surface. Mr. Foucault subsequently engaged in the same experimental investigation, with apparatus constructed upon an original principle. It is easily seen that if we have the true velocity of light, we have indirectly and at once the solar parallax. For the aberration of the fixed stars gives us a determinate relation between the velocity of light and that of the earth in its orbit. And the velocity of the earth with its periodical time, furnishes directly

its mean distance from the sun. The results obtained by both these experiments, inclined toward the smaller value. Mr. Foucault more recently resumed his experiments; and having taken extreme precautions to exclude the possibility of sensible error, he arrived at new results which led him to announce the parallax at length, at 8″.86. It is interesting to observe the near approach to identity between this result, and that reached by Professor Newcomb by a method purely astronomical. Professor Newcomb combines this determination with several others, including that deduced from the experiments of Foucault just mentioned, and those which have been furnished by the micrometrical measurements of Mars by Professor Hall made in 1862, the parallactic equation of the moon, the lunar equation of the earth, and finally the transit of Venus of 1769 recomputed by Professor Powalky; the conclusion arrived at, after assigning its proper weight to each determination, being, that the true parallax is 8″.848, or 8″.85, with a probable error of 0″.013. This would make the mean distance of the sun 92¼ millions of miles, or about 3 000 000 less than the value heretofore received.

A very curious question has been agitating the astronomical world for more than a year past, in regard to the asserted disappearance of the lunar crater Linnæus. This crater was described by Mädler as very large, being not less than six miles across, and also as very deep. In the latter part of 1866, it was announced by Dr. Schmidt of Athens, that it had entirely disappeared and had been replaced by what he described as a whitish spot. Subsequently within this spot and at its centre, it was further asserted that there had appeared a minute dark point, seen independently by Schmidt at Athens, Secchi at Rome, and Lyman at New Haven, indicating the formation of a new crater; and this has apparently grown larger and gradually changed its place. If these observations, which seem as yet to be received by many with reserve, should be confirmed, quite a new interest will be given to the study of the physical aspects of our satellite.

On the 21st of August, 1867, Jupiter was seen under those rarely occurring conditions in which he appears without satellites. Two of the satellites were in eclipse, and two were pro-

jected on his disk. At this time the appearances presented by the satellites in transit were so remarkable as to attract general attention. The light of the fourth satellite was extremely feeble so that it was described as appearing no brighter than its shadow on the disk. There was noticed, also, by one observer, a singular configuration of light and shade occupying one half the disk of the third satellite, such as had, on a former occasion, been seen on the other half. This suggests the possibility that the commonly received opinion, viz., that all the satellites of the planets like the moon, turn constantly the same face toward their primaries, is not true. The dimness of the fourth satellite in transit, and the varying brightness of all of them in similar circumstances, were noticed a century and a half ago by Pound; and the latter phenomena were at the time ascribed by him, very plausibly, to the proper rotation of those bodies on their axes and the unequal reflecting power of different parts of their surfaces. The identity of the rotation and revolution period of the secondaries must evidently no longer be taken for granted.

Of the family of small planets between Mars and Jupiter, the number rapidly increases. Since your last meeting, eight additional members have been announced, of which three are due to an American observer, Professor Watson of Ann Arbor. The total number now known, so far as I am at present informed, for it is not quite safe to be positive, is exactly one hundred. The earliest body of this class ever discovered, and except one probably the largest, was first seen by Piazzi on the very first day of the present century. It is interesting to remember, at a time when every few months adds a new member to this already very numerous group, that this first of the minor planets was lost soon after its discovery, and was not recovered again for nearly eleven months. Piazzi's last observation, previous to the loss, was made on the 11th of February, 1801, when illness interrupted his labors. The rediscovery was made by De Zach with the aid of an ephemeris computed by Gauss, who had never seen the object, on the 31st of December, of the same year. Of seventy one of these small planets Mr. Stone has formed a table of probable dimensions. He places Vesta, discovered fourth in order in 1807, and

esteemed at the time of its discovery the smallest of the four, at the head of the list, and fixes its diameter at 214 miles. Echo stands lowest in his catalogue, its diameter being estimated at only seventeen miles. It illustrates the increasing frequency of discovery in this class of celestial objects in recent years, and at the same time the high degree of perfection of the modern star maps which has made this frequency possible, that after the discovery of Vesta nearly forty years elapsed before another addition was made to the list, and that the first half of this century yielded in all, only ten. Since 1850, the average rate of discovery has been about three a year.

The period of the diurnal rotation of Mars has recently been redetermined by Mr. Proctor of the Royal Astronomical Society from a comparison of Hooke's observation made March 12, 1666, with recent observations in 1856, 1864 and 1867, the longest interval embracing 71 538 rotations. The result shows the necessity of a correction amounting to about a second, of the period assigned by Mädler from the results of seven years observations. It exceeds by about a tenth, or fifteen hundredths of a second, the determination made by Kaiser.

Our countryman, Mr. Cleveland Abbe, formerly of the Pulkova Observatory, has made a careful and laborious calculation of the parallax of Sirius, from the observations of this star with the transit circle made at the Cape of Good Hope. He concluded the parallax to be not less than 0″.17 nor more than 0″.37 making the distance to fall between six and thirteen hundred thousand times the diameter of the earth's orbit. The determination by Henderson, deduced from observations at the same point, lay between these limits and near the lower, implying a distance of 900 000 similar diameters. When it is considered that this star is by far the brightest in the heavens, exceeding, if we adopt the scale of Secchi, perhaps forty times the brilliancy of 61 Cygni, whose parallax is near the maximum given by Abbe in this determination, we may form some feeble conception of what must be the intrinsic splendor of this magnificent globe.

Mr. Abbe has also contributed to the Royal Astronomical Society an interesting examination of the distribution of the nebulæ. The clusters and planetary nebulæ he considers to

belong to the milky way, but other nebulæ form independent systems.

On the seventeenth of August in the present year, a day now near at hand, there will occur a total eclipse of the sun, under circumstances remarkably favorable to the duration of the obscuration. It is stated by Dr. Weise, before the Royal Astronomical Society, that there are but two total eclipses in history which, in respect to duration, can be compared to this. The first is the famous eclipse said to have been predicted by Thales, which happened in May, 583 B. C., during a battle between the Lydians and Medes, filling both armies with consternation; and the second was that which occurred in June, 1435, in Scotland, where it was long remembered as "the black hour." The eclipse now just at hand occurs near the perigee and almost at the moon's minimum distance from the earth. It happens also precisely in the moon's ascending node. Where the centrality takes place on the meridian, the two bodies will be nearly in the zenith, so that the augmentation of the diameter in altitude will be a maximum, and the rate of motion of the shadow on the earth's surface a minimum. Thus the total obscuration will be hardly less than seven minutes. It is unfortunate that this interesting phenomenon will occur in a portion of the earth least favorable to its observation. It commences in Abyssinia, crosses southern Arabia and Hindostan, the Gulf of Siam where the duration of the total phase is maximum, the islands of Borneo, Celebes, and the southern part of New Guinea to the New Hebrides, where it ends at sunset. The extraordinary opportunity which this eclipse offers for studying the phenomena of the solar atmosphere, and the nature of those extraordinary rosy flames, so called, occurring during totality, which have for thirty years past attracted so much attention and excited so deep an interest on the part of astronomers, will not, however, remain unimproved; very large and thorough preparations having been made by the astronomers of England and continental Europe, to secure the most complete record possible of all the phenomena, including photographic views of the phases from many different stations. The results obtained by such means during the eclipse of July 18, 1860, notwithstanding the dis-

appointments which attended some of the distant expeditions then sent out, were of great interest; and it is to be hoped that the similar opportunity which now presents itself will be a means of securing a rich addition to the materials in possession of science for the study of solar physics.

This question indeed of solar physics, the constitution of the sun's atmosphere, his photosphere, and his central nucleus, is one which has attracted to itself recently much laborious attention. The solar spots have been especially the subject of patient and persevering study, furnishing as they do evidence of the extraordinary activity and energy of the forces which are constantly in operation producing changes in the luminous envelope. Mr. De la Rue, Mr. Balfour Stewart and Mr. Loewy, have particularly devoted themselves to the determination of the laws governing the variations of the total area of spot-surface, and its distribution over the face of the sun. This area was ascertained for successive epochs by the actual measurement of enlarged photographs of the sun. The face of the sun being divided into thirteen equal portions or lunes, each corresponding to a day's motion, by great circles passing through its poles, the total area of the spots in each lune was ascertained for several series of observations, embracing a period of many months, and the results tabulated. From a series of elaborate tables thus formed, it was presently made apparent that the region of maximum spotted area is not always similarly situated on the sun's disk as seen from the earth, but passes from left to right. The existence of an external influence affecting the phenomenon is thus made very probable, and by comparing the changes with the movements of the planets, Venus seems to be distinctly indicated as the body exercising this influence. The recurrence of the maximum spot-area in the same longitude relatively to the earth on the sun's surface, is evidently periodical, and the period corresponds to the synodical period of Venus. Also the average size of a spot seems to be maximum on the side of the sun which is turned from the planet. A similar effect is ascribed to Jupiter, but the investigators show that this, though perhaps as powerful, is not characterized by so large inequalities as that of the smaller body, whose comparative

inferiority of mass is more than compensated by its proximity to the sun. The observers deduce the additional conclusion from their inquiry, that the spots are, on the whole, nearest to the solar equator when Venus is in the plane of that great circle, and farthest from the solar equator, when the heliographical latitude of the planet is greatest. This observation gives significancy to the fact long known, that the solar spots are never seen far distant from the equatorial region.

As to the cause of these spots, or more generally and more properly perhaps, the nature and determining causes of the photosphere in which they make their appearance, a conjectural theory must be sought in the known influence of exceedingly high temperatures and pressures upon the chemical condition of all bodies. This subject has been discussed from time to time with great ability, by Mr. Faye of the Imperial Observatory of France, by Mr. Balfour Stewart of London, and by Mr. Sterry Hunt of Montreal. These gentlemen conceive the sun to consist of an immense mass of matter in a gaseous condition, but with its chemical affinities held in check by heat, so that all the elements are mingled together but not combined. The condensation of this gaseous mass is such as to give it a density not less than that of water. Radiation from the surface of the mass produces a depression of temperature sufficient to permit certain elements to enter into combination; forming finely divided solids or liquids, which being held in suspension in the surrounding gases and vapors, become intensely luminous, as phosphoric acid is momentarily luminous when formed by combustion in oxygen gas, or as carbon is luminous in the candle flame. Supposing the general surface of the sun to be thus overspread with a luminous envelope, the theory of the formation of the spots connects itself with that of the lines of more than average brightness, or the faculæ. And it seems to be established that while these latter are in the nature of immense elevations or wave crests of the luminous matter in suspension, so the former are vast depressions. Or rather, while the faculæ are produced by ascending currents, the maculæ are formed by currents descending. The descending currents carry the luminous precipitate into a region of greater heat, where it is decomposed

and disappears. Above the photosphere there is evidence of the existence of an atmosphere, extending perhaps a million of miles, perhaps much more, above the visible surface, and in this, on those rare occasions furnished by total eclipses, are seen the irregular red clouds or projections which have been so much discussed of recent years, and of which we hope soon to know much more. These flames are to be examined with the spectroscope and the polariscope during the eclipse of this month, and their photographic images will be secured at many stations. Observations will also be made with a view to the detection, if possible, of the intra-mercurial planet, of which the existence has been suspected, and which was looked for in 1860 without success.

The star-shower of November 14, which had been witnessed in Europe on the 14th of November, 1866, returned, as predicted, one year later, in a more westerly longitude, and was extensively observed on this side of the Atlantic. This return has furnished fresh material to confirm the received theory of these displays. Periodical meteors may be regarded as proceeding from what are called by Schiaparelli cosmical clouds, which, from having a cometary character, have been transformed by perturbations produced by the planets into currents or streams, revolving round the sun in extremely elongated orbits, and tending constantly to become rings. Mr. Newton had pointed out that the period of revolution of the cosmical cloud producing the November meteors must be one of five, of which four were less than the earth's period, and one much larger, or thirty two and a quarter years. Soon after the return of the shower in 1866, Mr. Adams took up the investigation of the question, by computing the perturbing effect of the planets upon the movement of the node, on supposition, first of a period less than a year, and secondly, of one extending to thirty two and a quarter years. The first supposition gave less than half the observed amount of movement, and the second accorded so closely with observation as to leave no doubt of its truth. Mr. Leverrier regards this meteoric stream as having originated from the perturbation of a cosmical cloud or comet produced by Uranus about seventeen centuries ago.

A very curious fact connected with this subject, is the recent discovery that certain bodies which have been distinctly recognized as comets, are unquestionably members of periodical meteoric groups. It was found by Schiaparelli, that the great comet of 1862 is only one of the August meteoroids, and that the first comet of 1866 is a member of the November group, near the head of the train. Another curious fact, discovered by Hoek of Utrecht, is that there are double or triple systems of comets, or systems in which two or three must have had a common origin, though their perihelion passages have been separated in time by years.

Some observations on the spectra of the meteors of 1866 were made by Mr. Browning of London. Those of different bodies presented appearances remarkably different. They were generally continuous, some presenting all the usual colors except the violet, which was perhaps not seen on account of its feebleness. In some, the yellow was greatly predominant, though the other colors were present. Some were purely yellow, or nearly so, and a few purely green. This mode of observation, if pursued, may lead to useful results.

The spectroscopic observations of the sun, stars and nebulæ, continue to be prosecuted, those of the stars especially by Father Secchi. He has arranged the stars, as characterized by their spectra, in three groups, the first embracing only such white stars as have a black band in the part of the spectrum between the green and blue, and another in the violet; the second distinguished by possessing colored bands in the red and orange; and the third giving only fine lines. This last embraces Arcturus, Capella, and our own sun. To this, also, the red stars generally belong. Few stars are so exceptional as not to be referable to one or another of these classes, but one, at least, has been found by Secchi, of which the spectrum resembles that of the sun observed through an absorbing solution. This indicates an atmosphere of peculiar character and of extraordinary density.

To the metals discovered spectroscopically to exist in the sun, Mr. Angstrom adds manganese. This makes the twelfth, or if we call hydrogen a metal, the thirteenth, certainly ascertained to exist in our central luminary; and the presence there

of copper, is also probable. None of the precious metals are yet among the number.

Mr. Stockwell has recently published an extension backward of his chart of the eccentricity of the earth's orbit, for a second million of years. Should the theory discussed two years ago at Buffalo, which connects the glacial periods of geology with this secular inequality, be found tenable, the chart of Mr. Stockwell may serve a useful purpose in regulating geological chronology. An interesting inference deduced from the inspection of the chart, is that the periods of corresponding maximum and minimum eccentricity occur at intervals of 1450000 years. Another is, that two large maxima are sometimes immediately consecutive, and sometimes separated by a number of minor maxima, so that, on supposition that such a maximum brings a glacial period, the interval between two such periods, or two severe periods of such a character, may be very variable.

It seems due to the memory of one of the most deserving and most highly esteemed of American astronomers, now passed away, that I should mention in connection with these astronomical notices, the recent publication of the fifth volume of the Annals of the Harvard Observatory, containing the extremely interesting observations of the great nebula in Orion, made by Professor George P. Bond. This work completed and edited by Professor Safford, now of the Dearborn Observatory of Chicago, constitutes one of the most elaborate and valuable studies of a nebula that has ever been made; and it will be a standard work of reference in all future observations of the changes in form or brilliancy of this most beautiful object of its class, and of the stars associated with it.

In geology, while the contributions to particular knowledge have been numerous, and evidences of activity in every quarter are satisfactory, there is little which is new in regard to theory. Mr. Lombardini has communicated to the Royal Institute of Lombardy, some evidences of glacial action in the great depression of central Africa, and supposes that the mountains of Abyssinia and more southern regions will furnish confirmatory evidence to the same effect. He concludes, as Agassiz has done from his observations in Brazil, that during the glacial

period the ice-envelope overspread the entire planet. The tenability of this theory is now one of the questions of highest interest yet unsettled. There is some ingenuity in the suggestion recently made, that glacial phenomena in low latitudes as well as the evidences of a former tropical climate in the polar regions may be accounted for by supposing the upheaval of mountains in extra-tropical regions to have caused a gradual sliding of the earth's external crust upon the central fluid, in consequence of the centrifugal force of the protruded masses, tending to drag them down to the equator. It is certain that the amount of flexure which the crust would on this supposition have to undergo, would not be sufficient to produce rupture, and the suggestion is free from the mechanical difficulties which attend the hypothesis of a change of position of the earth's axis of rotation.

The geological researches of Mr. Raphael Pumpelly in Japan, Mongolia, and China, have at length appeared among the Smithsonian Contributions. They furnish a fund of valuable information in regard to an extensive region of the earth which has been little explored. Dr. Newberry's discovery of the mesozoic character of the great Chinese coal deposits, from fossil plants gathered by Mr. Pumpelly, is one of the most interesting facts brought to light by modern geological research.

Archæological inquiries, especially researches concerning the physical and social condition of prehistoric man, and the chronology of the several phases of his existence, continue to be pursued with unabated zeal. New traces of the primitive races are continually appearing, some of them in unexpected quarters, as for instance remains of pile-dwellings in the Thames at London, and flint implements associated with the bones of recent and extinct mammals at Paris. The most important novelty in the way of discovery in this department, however, is that of a human skull, in the lower postpliocene deposits of the plain of the Aretino in Italy. This occurs in a regular stratification consisting of lacustrine clays of great thickness, embracing bones of *Elephas primigenius*, *Bison priscus*, *Cervus euryceros*, and others. Stone implements were found associated with the skull and with these bones. Whatever may be said of the date of deposits like

organic compounds. A year ago, he effected the direct synthesis of acetylene by the union of carbon and hydrogen; and more recently, from acetylene, he has succeeded in producing oxalic acid. Another very interesting result obtained by the same investigator, consists in the discovery of a universal method of transforming any organic body whatever into a hydrocarbon, with the amount of carbon unchanged, and the hydrogen a maximum. The reagent which produces this effect is hydriodic acid.

The method of Berthelot just mentioned for producing oxalic acid synthetically, is equalled if not surpassed in interest by a discovery of Dr. Drechsel, by which the same acid is produced by a reduction of the carbonic. This result, on its announcement in the chemical society of London, was received with expressions of high and just admiration.

Considerable interest has also been excited by the researches of Mr. Griess, upon a series of organic compounds in which the hydrogen is replaced by nitrogen, which have led him to the discovery of a class of organic explosives, exceedingly by energetic. It is a curious fact that every known explosive, liquid or solid, is a compound into which nitrogen enters.

In physico-chemistry, a very ingenious theory has been proposed by Mr. M. Carey Lea, of Philadelphia, to explain the action of light upon photographic salts, especially the iodide of silver. The prevailing opinion has been, that the salt undergoes a partial reduction, and thus acquires the property of combining with the vapor of mercury. The improbability of this hypothesis is shown by the fact long known to photographers, that the plate loses this power of combination, and recovers its sensitiveness to light, if kept for a time in a dark place. Mr. Lea is of opinion that the change is physical and not chemical, and that the molecules acted upon by the light have their vibratory motions—their *vis viva*—exalted, by sympathy with the luminous vibrations; as sonorous bodies are excited by a sound in harmony with that which they are capable of yielding. He proposes to give to this influence the name actinescence. This theory serves to explain the remarkable phenomenon discovered by Niepce de St. Victor, viz:

these, which must be very ancient, there can hardly be a doubt that the computations which have been founded on the observed progress of the formation of alluviums in the neighborhood of former discoveries, have led to extravagant estimates of their antiquity. An interesting reëxamination of the phenomena presented in the valley of the Somme, has recently been published by Professor Andrews of Chicago..

Chemistry is so fertile a subject that it would be hopeless to undertake anything more than to notice, here and there, a point of interest in the history of its progress. Two improvements in technical chemistry are announced, which may, possibly, be found to be of great commercial, as well as of scientific importance. One of these is a method which has been protected by patent in France, for manufacturing sulphuric acid without the use of large leaden chambers. There is nothing new about the process in principle, but much in the details of application; the apparatus being forty times reduced in bulk, and the acid produced almost entirely free from impurities. Considering the importance of this great mineral acid—the most important probably, at once to chemistry and the arts of all chemical reagents—anything which tends to simplify the process or diminish the cost of its production, or to improve the quality of the product, must be esteemed a very essential benefit in which science and the industrial world share equally.

The other novelty referred to, is a substitute for the famous process of Leblanc, for the preparation of carbonate of soda from the chloride. In this the sulphuric acid is replaced by sesquioxide of chromium, heat being applied and a current of steam directed upon the mass. A second heating with charcoal, which converts the chromate into carbonate and revives the sesquioxide, completes the operation. The process is ingenious and beautiful. Whether it will prove commercially a success remains to be seen.

Contributions to organic chemistry have been too numerous to admit of a detailed review. In this department, Berthelot continues to stand preëminent. His labors on the hydrocarbons have been productive of many extremely interesting results, comprising among them the synthesis of several

organic compounds. A year ago, he effected the direct synthesis of acetylene by the union of carbon and hydrogen; and more recently, from acetylene, he has succeeded in producing oxalic acid. Another very interesting result obtained by the same investigator, consists in the discovery of a universal method of transforming any organic body whatever into a hydrocarbon, with the amount of carbon unchanged, and the hydrogen a maximum. The reagent which produces this effect is hydriodic acid.

The method of Berthelot just mentioned for producing oxalic acid synthetically, is equalled if not surpassed in interest by a discovery of Dr. Drechsel, by which the same acid is produced by a reduction of the carbonic. This result, on its announcement in the chemical society of London, was received with expressions of high and just admiration.

Considerable interest has also been excited by the researches of Mr. Griess, upon a series of organic compounds in which the hydrogen is replaced by nitrogen, which have led him to the discovery of a class of organic explosives, exceedingly by energetic. It is a curious fact that every known explosive, liquid or solid, is a compound into which nitrogen enters.

In physico-chemistry, a very ingenious theory has been proposed by Mr. M. Carey Lea, of Philadelphia, to explain the action of light upon photographic salts, especially the iodide of silver. The prevailing opinion has been, that the salt undergoes a partial reduction, and thus acquires the property of combining with the vapor of mercury. The improbability of this hypothesis is shown by the fact long known to photographers, that the plate loses this power of combination, and recovers its sensitiveness to light, if kept for a time in a dark place. Mr. Lea is of opinion that the change is physical and not chemical, and that the molecules acted upon by the light have their vibratory motions—their *vis viva*—exalted, by sympathy with the luminous vibrations; as sonorous bodies are excited by a sound in harmony with that which they are capable of yielding. He proposes to give to this influence the name actinescence. This theory serves to explain the remarkable phenomenon discovered by Niepce de St. Victor, viz:

that light may be "stored up" or absorbed by bodies which have been exposed to it, so that afterwards such bodies will act chemically upon sensitive plates in the dark; an effect which continues for a considerable time. The plausibility of the theory will doubtless secure for it a general reception.

Dr. Emerson Reynolds of Dublin, has recently shown that photographic plates which have been impressed by light, may be quickly restored to sensitiveness, and the latent images obliterated, by exposure to ozone. He regards this fact as a conclusive proof that the image is produced by a chemical change, and that what he calls the mechanical theory, which is the theory of Mr. Lea, is untenable. The premises do not seem to justify the conclusion. The molecules of ozone of which the density has been recently found by Regnault to exceed 1″.5, may very probably possess precisely such a vibratory period as to interfere with the vibrations of the molecules of the iodide, though the oxygen and nitrogen of the air have not such an effect.

It has been demonstrated by Professor Knoblauch that heat, like light, transmitted through doubly refracting crystalline films, suffers interferences; so that in the polariscope, if the analyzing prism is rotated, the calorific rays transmitted in opposite azimuths are complementary to each other. This furnishes a new and very interesting evidence of the fact that light and heat are physically identical, and that their differences are only differences relatively to our perceptions.

To the apparatus of static electricity a valuable contribution was made a year or two since, in the now well known induction machine of Mr. Holtz. A similar service was rendered to dynamic electricity in the ingenious magneto-electrical machine contrived by Mr. Wilde. At the Universal Exposition in Paris during the last year, Mr. Ladd, of London, exhibited a mechanical electro-motor of still more simple construction, in which the currents generated in the wire wrapping of a revolving armature are employed to excite the electro-magnet by which the armature is itself excited; while a second revolving armature is employed for the purpose of obtaining an independent current to be used in electrolysis or for the production of light.

This machine of which the effects are surprisingly powerful has excited an extraordinary interest for the promise it holds out of practical usefulness, and for the illustration it affords of the direct conversion of dynamic into electrical energy.

Two new chemical electro-motors have been announced, one by Mr. J. B. Balsamo formed of plates of iron for both positive and negative elements, immersed in cells with a porous partition, filled on one side with dilute acid, and on the other with solution of table salt. The iron on the side of the acid acts as the positive element, and the other as the negative. This battery which is of considerable power, has a theoretic as well as practical interest.

The other novelty is a battery in which the negative element is chloride of silver fused round a silver wire, the positive element being zinc. The pair are immersed in salt water. This apparatus which is recommended by its simplicity and neatness, is due to Dr. Hugo Müller and Mr. Warren de la Rue.

A curious addition has been made to the materials for the theory of electro-dynamics, in the discovery by Mr. Edmond Becquerel, of the fact that capillarity is an electro-motive power. The contact of solutions in capillary spaces is attended with the deposition of their bases in metallic form, precisely as in galvano-plastic operations. This discovery is eminently suggestive. Its farther prosecution cannot fail to lead to interesting, perhaps to important, results.

To dynamic electricity Dr. Edlund has also contributed the discovery that currents passing through conducting metals produce an expansion of the conductor which is independent of heat.

Certain phenomena observed by Professor Daniell show the existence of a considerable mechanical power in the voltaic current, capable of transporting heavy substances in mass. This discovery taken in connection with nearly simultaneous observations by Mr. H. Poggendorf on an analogous effect produced in the movement of static electricity, presents a paradox of which the solution is not obvious. In the voltaic current a conducting substance like mercury is transported rapidly along a tube from the positive to the negative pole. This movement will even take place on an inclined plane against gravity. In the continuous discharge between the electrodes of a Holtz

machine, however, the mercury moves with equal rapidity from the negative to the positive pole.

Discussion still continues on the much controverted subject of spontaneous generation, notwithstanding the seemingly conclusive experiments of Mr. Pasteur. In connection with this subject, Dr. Wyman's investigations with respect to the power of living organisms to resist heat, published since your last meeting, possess unusual interest.

Recent publications throw much light upon the question heretofore unsettled, as to the immediate source of the energy manifested in the muscular contractions of living animals. It has been a favorite supposition that this energy is furnished at the expense of the muscle itself, and that no part of it is derived from any other source. This hypothesis seems to have been conclusively overthrown, by recent observations; among which may be mentioned those of Dr. Douglass upon the prisoners in the Madras penitentiary, which show that the proteine compounds furnished in food are not an adequate equivalent for the work performed. Muscular energy must therefore, in a measure at least, be derived from the oxidation of non-azotized compounds contained in the circulation. But this information, though interesting, brings us no nearer to the solution of the mystery how heat or electricity or chemical force becomes muscular force in the living animal. On the subject of muscular dynamics, there is absolutely no theory at all. We have a rational proximate account of the conversion of electrical energy into magnetic energy. We have a conception of how and why the magnet produces motion. We have a theory also of the transformation of heat into molar force in the cylinder of the steam engine. But in regard to the contraction of animal muscles there is not yet in any proper sense, any physical theory whatever. We have on the one side the chemical action. We have on the other the mechanical. The connecting link—the mode of transformation of the one into the other—is wanting—so utterly wanting that the vaguest conjecture cannot hazard even a hypothesis to supply it. Have we not here probably reached a limit which it will never be permitted to human investigation to transcend? This question suggests a thought to which I desire, for a moment before concluding, to

draw your attention, and for the sake of which I have purposely abridged the imperfect sketch I have attempted of the recent progress of science.

The great flood of new truth which scientific investigation has in our own day let in upon the world, the multitude of mysteries of which but lately it would have seemed hopeless, perhaps presumptuous, to attempt the solution, but which have melted away nevertheless in the powerful focus of modern scrutiny, have given birth to a feeling, not yet perhaps universal but quite general, that there is no truth whatever which is not explicable by physical law; or, to state the proposition a little differently, that, whatever may be claimed as truth without being so explicable, is but idle fancy, the mere "stuff that dreams are made of," unworthy to be considered by the genuine philosopher. The doctrine, if I understand it, is that we know nothing but phenomena, that as to the relations of these we know only that they occur in certain invariable sequences, the idea of causation being a mere conceit, or at least a thing with which we have nothing to do; and that whatever transcends observation, or is beyond the reach of demonstration, must be set down as matter of useless speculation, as utterly and absolutely unknowable. Under the operation of this principle, metaphysics in the highest sense of the word, that is to say abstract philosophy, and ontology, or the theory of being, must be swept out of existence. So at first thought, it would seem, must be psychology; but this appears not to be so, and it is precisely here that I find the point to which I propose to direct my animadversion. The spirit of the so called positive philosophy very strikingly characterizes much of modern physical inquiry, even when conducted by men who repel the imputation of belonging to the school of Comte. It is in harmony with this spirit to exact that all phenomena, whether mental or material, shall be regarded as belonging to one common class, and shall be treated in precisely the same way. It thus builds up a new mental philosophy of its own; or reduces rather mental philosophy to be a branch of physics. I believe that this exaction is too comprehensive. I believe that physical inquiry has a field for its legitimate application which is limited by certain natural and definite boundaries. I believe that the

positive spirit itself properly applied within this field, leads us directly up to the conclusion that there is something certainly existent which it cannot reach, and which if not absolutely knowable is yet knowable in its most important relations; which is only to say that it is knowable in precisely the same sense in which we can be said to know anything whatever. That I may make myself more clearly understood, I must enter into some detail of illustration.

A very distinguishing peculiarity in the physics of recent years is the prominence which has been given to the doctrine of the conservation of force. A doctrine bearing this name has indeed long been recognized in mechanics, but it was earlier restricted to mechanical force alone; and as to force in that form even, was held to be true only under certain conditions, or with certain limitations. If the motion of a system of bodies is modified by their mutual action on each other, whether they draw each other by means of inextensible cords, or impel each other by means of rigid rods—the connections in either case being without inertia or weight—or whether they attract or repel each other by forces varying according to any law, or whether finally they act by direct collision, provided only they possess the property of perfect elasticity; in any of these cases the sum total of the living forces of all the bodies of the system will remain absolutely the same. This law was announced by Huyghens more than two hundred years ago. At that time force was understood in no other sense than as the power to move visible masses, or as the energy embodied in such masses when in motion. The truth was therefore rather abstract than practical. It admitted of no visible exemplification in terrestrial physics. No known solid possessed the property of perfect elasticity; and no experiments on motion could be instituted except in a medium by which motion is continually impeded. The mechanism of the heavens furnished the only perfect illustration in nature of this very important proposition. Notwithstanding the perpetually varying velocities of the great bodies composing the solar system, the consequence of their mutual actions on each other, and notwithstanding the incessant increase and diminution of living force to which each separate body is alternately subject, the

sum total of the living forces of the whole, remains from age to age unaltered. But in all dynamical phenomena which occur at the earth's surface, the truth of the law is masked by the unavoidable attendant conditions. Not even is the proposition of the conservation of force as understood in the time of Huyghens, illustrated in the motion of a single body; still less could its verification be looked for in the action of many bodies upon each other. A single body projected through the air, or rolled along a plane, soon parts with its motion. If in order to reduce the number of resistances, and simplify the experiment to the last degree, rotary motion be substituted for linear, and the experiment be conducted in a vacuum, the result only differs from the former by spreading out the waste over a larger period of time; the body still parts with its motion though more slowly than before. In the case of a body moving in a resisting medium—as a cannon ball for instance through the atmosphere, there may have been some early philosophers who imagined that the lost force still survived in the motion imparted to the invisible substance of the air; but in the experiment in which the sole sensible resistance is the friction of an axle upon its bearing, there is no doubt that all the world, men of science as well as those who claimed no such character, agreed in supposing that force was actually lost. The doctrine of the conservation of force, therefore, although the name originated two centuries ago, was not to the mechanical philosopher of that day what it is to us. It was a doctrine of conservation under favoring circumstances, and not of necessary persistence under all conceivable circumstances. Indeed while the notion of force was restricted to the mechanical energy exemplified in moving masses, it was impossible that the doctrine should be understood in any such absolute sense. For this species of energy, molar energy as it has of late become usual to call it, does disappear, as in the case of the turning wheel and the rolling ball, or as in the collision of bodies destitute of elasticity; so that if molar energy is the only form of force admitted, the doctrine of the conservation of force is not universally true. This doctrine therefore, in the comprehensive sense in which we understand it, is a doctrine of our own time, though it has borrowed its name from another

age. The term force has for us a much wider significancy than it had for the mechanical philosophers of the period of Huyghens and of Newton. It comprehends all those influences which were known to early physics by the name of the imponderables: powers concerning the nature of which there have been so many and so discordant hypotheses, which have been conceived of as fluids permeating the pores of the most solid bodies yet without increasing their weight; or as showers of minute projectiles pervading all space, yet without possessing momentum. Heat, electricity, light, are all regarded now as forces into which molar energy when it disappears is completely transformed; and to these we may add gravitation and chemical affinity. Magnetism, which in the books of the early writers occupied so prominent and so important a place, has ceased to take rank as an independent force, but is merged in electricity of which it is one of the manifestations. The facts that molar force may generate heat, and that heat in its turn generates molar force, are facts that have been long enough familiar: the notion even that heat is transformed force is by no means new. It may be said indeed that the experiments of Rumford, at the close of the last century, were sufficiently conclusive to establish this last doctrine. But even to admit this, was not yet to recognize the great truth of the conservation of force in all its fulness; since though it were regarded as true that force is converted into heat, it did not follow that all the force expended underwent this transformation. The water of a stream drives a mill, because the living force of the water in the race is transferred to the wheel; but a large part of this living force escapes without effect nevertheless. A certain mechanical force expended in friction produces heat. Does it any more follow in the one case than in the other, that the heat is the representative of all the force exerted? This could not be asserted until after very careful experimental investigation. Indeed we know that, in this particular form of the expenditure of force, it is not always true; though if the heat produced does not fully represent the force exerted, some other form of equivalent will appear — as for instance electricity.

The doctrine of the conservation of force, understood in the sense of declaring that no force is ever lost and no force ever

created, is one of which the truth could only be demonstrated after experimental science had reached a very high degree of refinement. And it could receive no experimental demonstration until there had been established some unit of reference to serve as a common measure of the quantities compared. If a given molar force disappears with the effect of raising the temperature of the body on which it has expended itself, and if the original *vis viva* and the resultant heat are equivalent in quantity, the same amount of force must be competent to produce always under the same circumstances, and in similar and equal masses, the same elevation of temperature, and different forces must produce effects of the same character in proportion to their amount. A satisfactory unit of comparison may therefore be found in the quantity of heat which is sufficient to raise a unit of quantity of standard matter, as for instance of water, one degree of the thermometer. With such a definite standard of comparison, it is possible to test the truth of the hypothesis that heat and force are convertible, and it is in this way that this important proposition has been experimentally established. The same exact equivalency has been in like manner shown to exist between electricity and heat, between chemical action and electricity, and between each of these and molar energy. And as light and heat seem to be but different manifestations of the same force, or different modes in which the same force is related to our perceptions, the law of conservation of force may be presumed to extend equally to both, and may therefore be regarded as the great law underlying and controlling all physical phenomena.

It has been affirmed, as I am aware, by a very distinguished metaphysician, Mr. Herbert Spencer, that this important truth is one which needs no demonstration. It is, as he asserts, a truth, which has its origin in direct intuition, and which lies at the foundation of all the knowledge we possess of the material world. According to him, indeed, we have no knowledge whatever which is not traceable back at last to force; and as to force he tells us that what we know of it is only, first, that it *is*, and secondly that it persists. In regard to the persistence of force, he finds the evidence of this intuition in the universal recognition of the postulate that action and reaction are equal.

This allegation, he holds, is but another form of saying that there cannot be an isolated force beginning and ending in nothing; but that any force manifested implies an antecedent force from which it is derived and against which it is a reaction. Clearly then he says, the persistence of force is an ultimate truth of which no inductive proof is possible. This exposition of the postulate is not perhaps entirely convincing. If reaction can be only understood as a taking up in living form by one body of a force which another body abandons, then Mr. Spencer's interpretation of it is true. But if it can also be understood as a destruction of any part of the original force, the consequence he deduces from it does not follow. To impute to it the former sense exclusively, is to reduce the postulate to an identical proposition, which in its proper form of expression would be this: Force which persists, persists. The truth is that this proposition, and all the laws of motion laid down by Newton, as well as the doctrine of the conservation of living force enunciated by Huyghens, were understood when announced, and long after, to be true only of forces acting in circumstances which are never realized at the earth's surface, and as theoretic and not as practical laws. They were admitted as deductions from observation, and not as self-evidently necessary. As we now understand the subject, Newton's third law is literally true under all circumstances, and it does in fact draw after it the necessity of the persistence of force; but this is a very different thing from supposing such a necessity to be a truth of intuition. The question of the origin of this notion is however of no practical importance: for the admission of the truth of the conservation of force as a theory does not render it any the less necessary that experiment should come in to inform us what are the quantitative equivalents of force under its various forms; and without a knowledge of these equivalents the doctrine can admit of no useful application as an aid to investigation.

I should not be justified in detaining you so long over a truth which in the physics of our day has become elementary, were it not that the bold extension which has recently been made of this law to the phenomena of conscious life, seems to me to transcend the limits which must ever arrest the progress of successful physical inquiry.

Besides the forces which we have been considering, and which are the only forces whose existence is made known to us in the phenomena of the inorganic world, there is another, whose effects are only seen in bodies endowed with life. Perhaps it should rather be said that there is a class of such forces (I call them forces for want of a better term) for the manifestations assume several very distinct characters; yet though the results are exceedingly conspicuous, the mode of operation is so obscure as hitherto to have baffled investigation. Under the influence of these forces compounds are built up which chemical art has in vain attempted to imitate, and which are of so noticeable instability that they break up spontaneously directly after the sustaining power is withdrawn. The bodies formed of these materials commence usually in germs of exceeding minuteness, pass through a regular cycle of growth and decay, and are at last abandoned by the mysterious principle which has given them their form and their structure, and left to perish. Among these bodies which make up the empire of organic life, two broadly distinguished kingdoms present themselves, the vegetable and the animal. Both of these are equally animated by the force which determines organization; but the former except in this particular do not differ from the brute matter out of which they spring; while the latter possess the power of self movement, and a will which directs this power. They are moreover influenced in their movements by impressions made upon them from without; impressions which we call sensations and refer to a property which we denominate sensibility. If we consider the animal kingdom more in detail, we shall see that it embraces classes widely differing from each other in point of dignity. Voluntary motion and sensibility in its lowest form of manifestation — the sense of touch — seem all that distinguish the humblest of these classes from vegetables. As we ascend in the scale, other senses appear, and a distinct intelligence with the evident exercise of judgment and memory displays itself; while the manifestations of emotion, as of pleasure, apprehension, anger, etc., are very obvious. These mental and emotional qualities increase in the energy of their development through many successive grades, until at length, leaping a great gulf of division which separates the highest

order of animal life from all other forms, they blaze out in man in the full effulgence of a perfected reason. Nor is it only the mental superiority of the human race which gives it its supremacy in the animal world. To deduce consequences from premises, to follow out a chain of causes to its probable results, is a high endowment, but it is one which is partaken though doubtless in a degree infinitely inferior, by many animals. But to distinguish between right and wrong — to conceive even the notion that there is a right and a wrong, argues the possession of an entirely new faculty. It matters not upon what basis ethical writers may choose to build up their systems, denying often the existence of such a thing as an innate moral sense. It matters not by what means they may explain the genesis of the ideas of right and wrong as originating in conceptions purely intellectual — the fact remains that man has a sense of right and wrong which is sustained by a faculty in him which he calls conscience, — a faculty which he knows to be a very different thing from simple understanding: and no evidence has ever yet appeared that such a sense or such a faculty exists even in the most embryonic condition in any other living animal.

We have then here several different forms of force associated with life: the organizing force; the force represented by sensibility; the force of will; the force of intelligence; the emotional force, and the force which manifests itself through the moral sense. If to all these principles I apply indiscriminately the word force, it is partly because of the poverty of language which furnishes no other generic term under which to embrace them all; partly because one of them at least, the organizing force, has long been distinguished by that name, though its existence as a physical force has been generally denied; and partly because when it is claimed, as it is of late claimed, that sensation, thought, conscience, emotion and will, are only manifestations of known physical forces acting upon the living organism, and are in fact those same known forces presented under new forms, these functions of sentient and intelligent life are by the terms of the proposition itself reduced to the class of physical forces and are properly called by the same name.

The question is, are we compelled to concede this claim. In accepting the doctrine of the conservation of force have we no choice but to admit that the barrier so long supposed to exist between the spiritual and the material world is a sheer creation of the imagination, and that the mind is as literally a machine operated by strictly physical forces as a wind-mill or a steam engine? Such an admission is by no means flattering to man's self-esteem. It is nothing less than revolting to his religious instincts. What are the arguments on which we are expected to accept it?

In regard to the processes of organic growth and development, of the assimilation of food, the absorptions, secretions and other physiological functions continually in activity in the living animal or vegetable, it may be admitted without hesitation that the efficient force producing the corresponding changes is derived from without, and that the changes themselves are the strict equivalents of the force thus expended. Take the case of the growing plant. Its food is mainly carbon, derived from the carbonic acid of the water which it draws up from the earth, or finds in the air around it. The force by which this very stable compound is decomposed, so as to leave the carbon free to combine with the plant, is furnished by the sun's rays. Of the equivalency of the two forces there can be no doubt; nor have we any need in the case to call in any special force under the name vital, to perform any part of the work of the change. And yet we certainly have need of something more than the mere juxtaposition of the materials in presence of the solar influence to produce the effect. If the principle of life is not in the plant, the operation will not proceed. The carbonic acid may be there, the vegetable tissue may be there, and yet the solar rays may play upon them forever without producing the slightest effect. The vital principle then is the something which causes the plant to grow. I will not call it a force — I think the term vital force a misnomer — because there is no work done for which we have not other forces in full equivalent — but I say that whatever it is, its presence is a necessity to the performance of the work, and in its absence the work is not performed. More than that, I say that not only will not the forces which produce growth during the life of the plant,

be sufficient to do so after life has ceased; but no combination of forces or influences or materials which human skill can contrive can now or will ever produce one leaf of the simplest plant or one blade of the humblest grass of which nature under the influence of the principle of life is producing millions on millions every succeeding summer.

More than this, there is a peculiarity of the compounds which are formed during the growth of organized beings, animals as well as plants, which marks a wide departure from the mode in which the forces of nature act when left entirely to themselves. The tendency of these forces is to equilibrium. Upon an irregular plane a heavy body, unobstructed in its motion will seek the lowest level. A balance, when disturbed, may oscillate for a time, but it comes at length to rest. Waters make their way to the ocean. Unequally heated bodies placed near each other share at length between them the common stock of heat. Electricity accumulated in excess upon one body will escape to others less highly charged. So too of chemical forces. They tend ever to produce the most stable compounds. A stronger combination never spontaneously breaks up to give place to a weaker. Suppose that all the materials of which the earth is composed should be thrown together in elementary form, it is possible that by the first action of affinities many feeble combinations would be produced; but it is certain that these must one after another give way to stronger ones until the whole should be combined in forms possessing the absolute maximum of stability, unless the process should be arrested by a solidification of the mass preventing farther motion. Now in the compounds produced during the growth of plants and animals, there is a complete inversion of this process: that is, there is an ascension from the lower to the higher level — a substitution of the weaker for the stronger, of the unstable for the stable. And animal compounds, that is those formed where the type of life is highest, are, as a rule, greatly more unstable than vegetable. The presence of the principle of life in organized bodies, therefore, determines the physical forces which in such bodies do the work of change, to operate in a manner in which they do not operate where it is absent. Light and chemical affinity, for

instance, are interchanged in the plant. Light disappears, and in disappearing restores to activity the force of affinity which was dormant because satisfied, but in order to do this it needs a determining influence which it finds in the simultaneous presence of the vital principle, and never without. Nor need it be said that in exercising such a determining power, life becomes a form of physical force. The light consumed and the chemical force revived are complete equivalents. Each represents the other. Neither has derived any increase nor suffered any diminution from association with the vital principle. What this principle has done is to determine, in regard to two forms of the same force possible in the same organism, which shall be the form manifested and which the form suppressed.

But if we are not to regard this influence of the vital principle as of the nature of a force, it will perhaps be demanded how it shall be explained? That, I reply, is a question which admits of no answer. It is a question which cannot be answered now, and a question which will not be answered ever. It is an inquiry which leads us beyond the limit of legitimate philosophical research. The vital principle differs from every form of force known to us, and from every other known property or quality, in that it confers upon the body which it animates a special character of individuality, and in that it is incapable of being insulated or of being transferred from body to body. We know it only through the peculiar organizing power which belongs to it, and which is manifested not merely in the chemical changes which it determines, but in the external forms which the resulting compounds assume.

The phenomena of vegetative life present us then with an inscrutable mystery, but they suggest no necessary conflict with the great doctrine of the conservation of force. But when we advance to the higher manifestations of life we are told that such a conflict does threaten us, unless we yield up all that we have been taught to believe of the possibility of spiritual existence, and relinquish, as untenable, convictions which have been partaken by the whole race of mankind. Organic changes are physical effects, and may be received without hesitation as the representative equivalents of physical forces expended. But sensation, will, emotion, passion, thought, are in

no conceivable sense physical. That they may be excited by the impressions of physical forces upon the animal organism is indeed admitted; that they are so is a matter of common experience; but that they are these forces themselves transformed into consciousness, and that in ceasing to be what they are they become physical forces again, is a supposition so totally repugnant to our instinctive convictions, that at the first announcement one is at a loss to conceive how it would ever have been for a moment entertained. This however is a philosophy which at the present day is boldly taught in public schools of science, and which numbers among its disciples many very able men. One of its most distinguished teachers and oracles, Mr. Herbert Spencer, sums it up briefly and tersely in these words:

"Various classes of facts thus unite to prove that the law of metamorphosis which holds among the physical forces holds equally between them and the mental forces. Those modes of the unknowable which we call motion, heat, light, chemical affinity, etc., are alike transformable into each other and into those other modes of the unknowable which we distinguish as sensation, emotion, thought: these in their turn being directly or indirectly retransformable into the original shapes. That no idea or feeling arises save as a result of some physical force expended in producing it, is fast becoming a common place of science; and whoever duly weighs the evidence will see that nothing but an overwhelming bias in favor of a preconceived theory can explain its non-acceptance. How this metamorphosis takes place, how a force existing as motion, heat or light, can become a mode of consciousness—how it is possible for ærial vibrations to generate the sensation we call sound, or for the forces liberated by chemical changes in the brain to give rise to emotion, these are mysteries which it is impossible to fathom. But they are not profounder mysteries than the transformation of the physical forces into each other. They are not more completely beyond our comprehension than the natures of mind and matter. They have simply the same insolubility as all other ultimate questions. We can learn nothing more than that here is one of the uniformities in the order of phenomena."

Nothing need be added to the explicitness of this statement. In every school of philosophy, the union of mind with matter has been among the profoundest of mysteries. There have been teachers who have held that mind itself is material, but even such have not assumed that thought is matter also. Thought has been regarded as a property, an endowment, a quality of the matter of which mind is made. The modern

school which inculcates the dogma that mind is force, is no less materialistic than that of Hobbes or Spinoza. We know nothing whatever of force but as a concomitant of matter. Without the existence of matter external to us, we should know nothing of force. Mr. Spencer, indeed, regarding the question from an opposite stand-point, lays it down among his fundamental propositions, that without the existence of force we should know nothing of matter; but his very illustration of this proposition shows its fallacy, or at least the very equivocal nature of the evidence on which it rests. For the force of which, as he correctly says, we first become conscious, is the force of resistance, perceived when the organs of touch encounter a material substance. Yet no such resistance could be felt unless the organ itself were material, and hence it is the preëxistence of matter which is the indispensable condition of the discovery of force. The two ideas in fact originate together. Resistance discovers to us the existence of matter and of force simultanequsly; and hence it is that we may reaffirm that we know nothing of force and can conceive nothing of force except as it is a concomitant of matter. The force of resistance is a dead force. Living force is a property of matter in motion. In those forms of the unknowable, so called by Mr. Spencer, which we name heat and light, modern philosophy recognizes the motion of matter, no less than in the energy of a projectile thrown from a cannon, or of a planet revolving in the celestial spaces. When heat was recognized as a force it was declared to be a mode of motion, and this descriptive title was by common consent received as fit and proper. Light, electricity, chemical action, are regarded no less as modes of motion also. In the case of these unknowables, the motion is presumably molecular, and in contradistinction to this, the force of moving masses is called molar; but in the case of all known forces, however unknowable they may be and however unknowable as to their essential nature they are in their mode of efficiency, they are simply matter in some mode of motion. The philosophy therefore which makes thought a form of force, makes thought a mode of motion; converts the thinking being into a mechanical automaton, whose sensations, emotions, intellections, are mere vibrations produced in its material sub-

stance by the play of physical forces, and whose conscious existence must forever cease when the exhausted organism shall at length fail to respond to these external impulses. If the law of conservation of force is therefore to be extended to mental phenomena, the immortality of the soul can be no longer maintained. On this hypothesis indeed man has no soul. Life is but a momentary phenomenon, a casual condition of matter, to be classed with combustion, incandescence, sound, odor, anything most accidental and evanescent. More than this, the living being, while his brief consciousness endures, is the mere sport of forces foreign to himself. His conscious freedom of will is nothing but an illusion. His thoughts, his feelings, his acts, are all links in a chain of inevitable events, determined by unalterable physical laws. He ceases to be a moral agent or an accountable being. Let it be observed moreover that the doctrine of necessity here forced upon us, differs from that which has been inculcated by necessitarian philosophers heretofore. The necessity which fetters the will by making it the slave of motive, is one in which the coercion is moral and not physical; and which leaves us room at least to respect poor human nature though we may compassionate. That which presents will and motive together as two modes of motion, of which the first is but the second under a new form, is a necessity of which the slave has lost even respectability, and is reduced to the humble level of a piece of mechanism.

But it is not because of these consequences that I reject a doctrine so derogatory to the dignity of humanity. The business of the philosopher is to follow on the trace of truth whenever it may lead. He must not suffer feeling, or preference, or prepossession, or prejudice, for a moment to bias his judgment. If the law of the conservation of force, rightly interpreted, conducts us of necessity to materialism, we must accept the conclusion, however humiliating we may find it to our pride or however ruinous to our hopes. To my mind no such necessity exists.

In proof of this position it is of course no argument, and I do not present it as one, to allege the utter incongruity which every mind uninfluenced, I was about to say unperverted, by

favorite philosophical preconceptions, feels to exist between mental and physical phenomena. Consciousness, to any mind, is a mystery sufficiently profound; but to suggest that it may be only a mode of motion, is to an ordinary mind little less than a self evident absurdity. But, as just remarked, this is no argument.

It *is* an argument, however, to say that thought cannot be a physical force, because thought admits of no measure. I think it will be conceded without controversy that there is no form of material substance, and no known force of a physical nature (and there are no other forces) of which we cannot in some form definitely express the quantity, by reference to some conventional measuring unit. Even while heat and light and electricity continued to be regarded as independent and unconvertible forces, they were still subjected to measurement, each after a manner peculiar to itself; and the ratio between the amount of latent heat of a pound of water and that of a pound of steam was as well known as it is now. The minutest quantities of electricity were determined by the most delicate of balances, and the intensities of different lights were numerically expressed by means of experimental and instrumental comparisons. Now no such means of measuring mental action has been suggested. No such means can be conceived. It is not a sufficient reply to this difficulty to say that thought is unlike any other species of quantity known to us, and therefore no such species of quantity can furnish us with a measure. We do not ask that it shall be measured against anything unlike itself. Heat was not originally so measured nor electricity nor light. A unit for the quantitative comparison of each of these powers, was found in a determinate quantity of the thing itself which was to be measured. And before thought can be claimed to be a form of physical force, it must become possible so to define a given quantity of thought, that all mankind, or if not all mankind, every scientific investigator at least, shall be perfectly able to understand exactly what quantity is meant. Can this be done? If so it will probably be in the same way in which units of reference have been established for other powers. Such units have been fixed by considering intensity, time of action, quantity of matter acted

upon, velocity generated, or work done; or by a portion of these elements variously combined. The force of gravity is measured by quantity of matter, time and velocity. Heat is measured by quantity of matter and determinate elevation of temperature. Light by intensity at a determinate distance. Now no one would think of measuring thought by the effect it produces on matter external to the thinker, which is *nil*. Of all the possible indications of greater or less of which the case admits, there are but two — intensity and time of action, — which are not self-evidently, at the very first suggestion, unavailable. Nor would these two alone suffice for a standard of *general* reference, even were the force one unquestionably susceptible of measurement; since quantity depends not only on intensity and time but also on the magnitude of the source. For a given individual this consideration might be disregarded; and the quantity of the *thought-force* of such an individual might be measured by the intensity conjoined with the time, but this only on supposition that thought-intensity is susceptible of estimate. On the impossibility of such an estimate it is hardly necessary to dwell. Now I maintain that a thing which is unsusceptible of measure cannot be a quantity, and a thing that is not even a quantity cannot be a force. I know it may be replied that though the unit of measurement may not yet have been found, it may nevertheless be so hereafter. I answer no; the objection is not merely that such a unit is at present impossible, but that even the conception of such a unit is an impossibility. Not only is the quantity of thought in a human brain presently unmeasurable, but its measurability is not even conceivable. And yet if thought is physical force, the time must come when its absolute energy at any given moment, and in any given individual, shall be expressible not only in units of the same description of force, or thought-units, but equally in equivalent units of any other force; so that perhaps we may be able to say of the mental labor of a philosopher in his study, that in half an hour it amounted to fifty or five hundred thousand foot-pounds.

Perhaps it may be said that there is an indirect way of arriving at the unit of thought-force which I have overlooked; that this force is doubtless generated by the oxidation of the

brain, that the amount of this oxidation may be ascertained during a determinate period of continuous mental labor; that this oxidation is a chemical action capable of producing a calculable amount of heat, and that this amount of heat may be properly accepted as the measure of the thought-force. This notion will not bear examination. Oxidation is going on at all times in all parts of the body. It is necessary to the maintenance of the animal heat. It is necessary to furnish muscular force. It is necessary to the large involuntary movements such as those of the heart the arteries and the intestines, and those smaller movements which accompany the various secretions. It is necessary especially in the brain to supply the nerve force, whatever it is — probably electricity — by means of which the mind transmits its mandates to the members, and through which it receives the impressions made upon the organs of sense. To distinguish between these various sources of consumption would be a problem of the most complicated character; and the difficulty or impossibility of securing adequate data for its solution by the observation of functional changes in the living man places it practically beyond the reach of investigation. But while this difficult problem remains unsolved, to assume that *if* solved it would show an outstanding balance of force to be placed to the credit of mental action is simply a *petitio principii*, and has no title to respect as an argument. The very strong probability is that no such balance would appear.

It is asserted, however, that some mental impressions are in a manner measurable; that this is true at least in regard to sensations. The feelings of pressure produced upon us by heavy bodies are greater or less in intensity according as the bodies are heavier or lighter in the balance. Bells, vibrating strings, wind instruments, yield sounds which we perceive to vary in loudness according to the degree of force expended in producing them. Bodies at different temperatures, as shown by the thermometer, affect us more or less forcibly with the sensation of heat. To all which it may be replied that sensation and thought are two things essentially distinct, and that we are speaking here of thought and not of sensation; that sensation is an affection of the organ of sense as well as of the

mind and is not exclusively mental: that the mental part of it is a cognition of the state of the organ, as modified by a physical force; and that the cognition embraces of course not only the fact but in a general way the degree of the modification; and therefore that a certain quantitative relation between sensation and external force is naturally to be looked for, and in no manner whatever sustains the hypothesis of a conversion of the force into the sensation. As the mind estimates quantitatively a phenomenon of the external world of which it receives information through the sense, so in sensation itself it estimates quantitatively a phenomenon equally external to its own essence though occurring within the material organism with which it is connected. It may furthermore be added that sensation is not, like thought, a thing of which the idea of quantitative measurement is in all cases inconceivable; for though the organ of sense may appreciate differences imperfectly, it s sometimes not very difficult to contrive modes of fixing a determinate unit of measure. This is obviously practicable in the case of sound; it has been actually done in the case of light. Sensation is produced by external disturbing forces. It is attended no doubt with corresponding internal changes. The case is analogous to that of the organizing process, or that of the building up of the structure of the animal or plant. The interchange which takes place between the external and the internal forces takes place without loss or gain to either; but it takes place in a manner which is determined by the presence of the vital principle; and to the mystery which attends the simpler process of organization, it here superadds a new and still more perplexing mystery, the phenomenon of consciousness.

But I proceed farther to say that purely mental impressions cannot be transformations of physical forces, because the character and the intensity of the impressions are not determined by, nor in any manner proportional to, the nature or the amount of the force impressed. The most powerful mental impressions are those which succeed to impulses affecting the nerves of sight or hearing; and these are sufficient often to call into activity all the muscular force which the percipient is capable of exerting. Such impulses, considered as forces, are too feeble, even when most energetic, to admit of being ex-

pressed as finite quantities. Yet if we interpret the phenomena in strict accordance with the hypothesis under consideration, we must say that these impulses are first transformed into mental force, and that afterward this mental force, by a new transformation, takes the phase of muscular and finally of molar energy. Thus understood the doctrine is manifestly absurd, since it makes a small force equivalent to a large one. To evade the difficulty, its advocates introduce here a new and supplementary hypothesis, according to which, while the mental impression is the equivalent only of the external physical force, the more palpable manifestations which follow come from the chemical action excited in some confessedly incomprehensible manner, by the instrumentality of the mental impression. But this explanation is totally unsatisfactory, and is hardly less than fatal to the hypothesis it is intended to subserve. Whatever claim to acceptance the new theory possesses must be founded on the promise it holds out of a solution of a mystery which the spiritual theory candidly recognizes as above solution. This promise it fulfils only by presenting the original mystery under a novel form. How to explain the connection of mind with matter is the problem ostensibly proposed; and a solution is assumed to have been furnished when mental forces have been reduced to the level of physical. But how to explain the influence of the will over the members, or the power of emotion over the muscular system, this is the true difficulty; and this the solution offered does not reach in the least. We all admit that the sensible displays of force of which animals are capable, are derived from the chemical action which takes place in their organisms, and are examples of the transformation of one form of force into another in conformity with the law of conservation. But the new philosophy does not pretend that *these* forces are the equivalents of the mental forces which have set them into activity; or that they have either wholly or partly passed through the form of mental force previously to their appearance in the mechanical form. How then does the mental force—that is, the will or the emotion—excite them to activity? That is the question of which we had a right to expect a solution when we were told that the doctrine of the conservation of force was to clear up for us all the difficulties of

mental science. And that question is left precisely where it was before.

An analogy I know has been intimated between this case and that of the explosion of a mine by the application of a match, or the discharge of a gun by the pulling of a trigger—examples in which a very slight force may suffice to evoke another of tremendous power. This argument would perhaps be entitled to some respect, if an attempt had been made to point out any conceivable mode in which, in the case in hand, the match is applied or the trigger is sprung. In the absence of any such attempt, it amounts to nothing more than pure hypothesis. It is simply another form of asserting that the connection of the phenomena is physical because it *must* be physical. It is once more a naked *petitio principii*. We have here a repetition of the difficulty which presents itself in the case of vegetable growth. In the presence of the principle of life there occur sequences of phenomena which do not occur in its absence. The facts are undeniable, but a rational explanation of them is as far off as ever.

Another consideration here presents itself which is of profound significancy, and which to an unbiassed judgment can hardly fail to be decisive. Mental impressions excited by impulses affecting the nerves of sense, take their character and are determined in their intensity not in the least by the force which those impulses represent, but by the *ideas* momentarily associated with them. An insulting expression addressed in English to a Frenchman unacquainted with that language will be heard with indifference, while an Englishman in the same circumstances will be roused to indignation. Any man may contemplate a dagger in the hand of a friend as coolly as he would do any other piece of cutlery; but if he sees the same weapon uplifted over him by an assassin, he will be filled with alarm, and will instantly put forth all the strength of which he is possessed, for the purpose, if not of resistance, at least of escape. In these examples the physical forces in the cases contrasted are as nearly equal as possible; but the mental impressions which they produce are widely different. There is no explaining these discrepancies consistently with the hypothesis under consideration. There is in either case but a given

LIBRARY OF THE UNIVERSITY OF MICHIGAN

amount of external physical force; there can be only the same definite equivalent of internal mental force. In the first instance the collocation and sequence of impulses on the ear of the Englishman conveys a *meaning* which in the case of the Frenchman is wanting, and the mental impression is consequently more powerful. Shall we say that this meaning—this accident accompanying the sounds—is force also? If so, how is it projected into the mind of the hearer? And how into one mind more than into another, when the physical impulses are in both cases identical? Shall we find any help in this difficulty from the doctrine of the association of ideas? What is the association of ideas? Is it anything more than a *habit* of consecutive occurrence, acquired by the frequent juxtaposition of the ideas in the order of time? And can unconscious forces be trained to form habits of diversity of action under physical conditions entirely similar? Of course there is no escape from this difficulty, but by assuming, as the advocates of this theory do assume, that unconscious forces become forms of consciousness; but this assumption involves the inevitable consequence that, in undergoing this transformation, they cease to be distinguished by that invariability in their modes of action which is the distinctive characteristic of physical force. Consciousness indeed is the rock on which this theory splits. Its supporters make no effort to explain this marvellous phenomenon. They admit, and indeed proclaim, that no explanation is possible. We make the same admission, but we claim at the same time that it is clear enough what it is not. The very fact that we can trace an unbroken series of entirely intelligible effects all the way up to the very point at which this surprising phenomenon presents itself, and then suddenly lose the thread altogether, is sufficient evidence that it is not physical.

But I will not pursue the argument. No possible *explanation* of mental phenomena can be founded upon a hypothesis which attempts to identify them with physical forces; but on the other hand the attempt leads inevitably up to the conclusion that there is something existent which is beyond the reach of scientific investigation—something of which, nevertheless, the existence is just as certain as its nature is inconceivable.

It is the fashion, I know, in the school of the positivists, to

treat as unreal whatever is undemonstrable. But the positivist believes in force, although he cannot tell what it is. And the organic world furnishes just as conclusive evidence of the existence of an influence superior to force, as the physical world exhibits of the existence of force itself. If indeed in accepting the doctrine of the identity of mind and force we could rid ourselves of mystery, we might find in such a fact some plausible argument for insisting on its admission. But in this respect it fails to advance us a single step. When it is objected that we can form no conception of mind, we reply that it is just as much out of our power to conceive of matter. If it is an inexplicable mystery that mind should act upon matter, the action of matter itself upon other matter is a mystery just as profound. It is certain that no two atoms touch each other. How then can many atoms cohere to form a solid mass? Even the great intellect of a Newton confessed itself baffled in the endeavor to conceive in what manner the sun could influence the planets, acting through a vast void space measured by millions of miles. Yet precisely the same difficulty returns in regard to every two particles which concur in the formation of the minutest material substance. It was a conception of Dr. Young that if one hundred men should be distributed equally over the surface of England, the distances between them might not be more disproportioned to their own dimensions than the void spaces in any solid to the atoms composing the solid. Does it then explain in any manner how mind acts upon matter, to say that mind is force—that is, that it is matter in motion?

The reply may be supposed, that the action of matter upon matter is a fact of observation; and that we must therefore accept it as a fact, and because it is a fact, although we cannot comprehend it. But the action of mind upon matter is equally a fact of observation, and not only that but a fact of consciousness also. Thus we gain nothing whatever even in the way of simplifying our philosophy, by contradicting our intuitions, resisting our instinctive convictions, and abjuring our faith.

In what I have said it has not been my design to obtrude upon the Association any theological dogma, or to advocate

any religious creed. With such subjects this Association as an Association has nothing to do. I have not indeed, here or elsewhere, been in any manner instrumental in introducing questions of religion or of abstract philosophy into discussions of physical science. But when such questions are introduced by others, and when it is demanded of us to pronounce as physicists that spiritual existence is an absurdity and religion a dream, it seems to me that no choice is left us but to proclaim our dissent, or be understood by our silence to accept the doctrine as our own. When such is the alternative, for one I feel bound to speak, and to declare my conviction that as physicists we have nothing to do with mental philosophy; and that in endeavoring to reduce the phenomena of mind under the laws of matter we wander beyond our depth, we establish nothing certain, we bring ridicule upon the name of positive science, and achieve but a single undeniable result, that of unsettling in the minds of multitudes convictions which form the basis of their chief happiness. If my views are correct, there is certainly a field which it is not the province of physical science to explore; and which, if we are wise, we shall carefully refrain from invading. Either this is so, or man himself is but a transient unmeaning phenomenon, brought into existence without a purpose and without a destiny; and neither science nor any other human interest is worth pursuing. In conclusion, gentlemen, thanking you for the kind attention with which you have listened to me, permit me to congratulate you on the cheering auspices under which you are once more assembled. You are here in a strength which recalls the happy days when your Association was in the zenith of its prosperity and its usefulness, and which justifies the hope that a fresh career of still more fruitful labors and of higher services to humanity is before it. May your mutual intercourse be productive of all the gratification which you have anticipated, and may the interchange of views in which you are about to engage bring with it at the same time enjoyment and profit.

PROCEEDINGS

OF THE

CHICAGO MEETING, AUGUST, 1868.

COMMUNICATIONS.

A. MATHEMATICS, PHYSICS, AND CHEMISTRY.

1. On the Application of Electricity to the Maintenance of the Vibrations of the Tuning-fork, and of the Tuning-fork to the Excitement of Vibrations in Cords and Threads. By Joseph Lovering of Cambridge, Mass.

The application of electricity to maintaining the vibrations of the tuning-fork is not entirely new, though it is of recent discovery; and the use of tuning-forks in producing sympathetic vibrations in threads or cords is not new, though it is of recent discovery. The tuning-fork, now exhibited to the members of this Association, was made from a bar of iron three-eighths of an inch in thickness, two inches in width, and sixty inches in length. Having great mass relatively to its surface, it is able, even without the help of electricity, to preserve its motion for several minutes, unchecked by the resistance of the atmosphere, and is, therefore, peculiarly fitted for the purpose of communicating corresponding vibrations to attached threads or cords. Nevertheless, these vibrations are slowly dying out. The length of the magnet is horizontal, the plane which passes through the two prongs being vertical. A magnet of the form of the letter U is so placed, that one of its branches is above the upper prong of the fork, and the other branch below the lower prong of the fork. This magnet consists of a soft iron core, wound with covered copper wire. When a current of

electricity is sent into the wire, the iron is magnetized and attracts the prongs of the tuning-fork. If the current acted constantly, the magnetism would retard the prongs when approaching as much as it accelerated them when separating from each other. This retardation is prevented by interrupting the electrical current and suspending the magnetic action when the prongs are approaching one another. This is accomplished by making the longer prong of the tuning-fork a part of the circuit. The upper surface of this prong has a little platinum or silver plate near its extremity, which is just touched by a platinum wire, attached to a spring and supported by a pillar, when the tuning-fork is at rest. After the prongs have moved outwards, the platinum point leaves the plate, the current is interrupted, and the prongs spring together without any retardation. A Grove's battery of six elements arranged, two for quantity and three for intensity, is sufficient to start the tuning-fork when at rest, and to put it in such energetic vibration that the extremities of the prongs strike the poles of the magnet, though at the distance of three-fourths of an inch, when the tuning-fork is at rest. Let one end of a cord be attached to either prong of the tuning-fork, while the other end passes over a pulley and carries a weight of sixteen ounces. If the length and diameter of the cord are such that, with this tension, its rate of vibration agrees with that of the tuning-fork, it will be thrown into a powerful sympathetic vibration. If the weight is reduced to four ounces, the cord will be tuned to the next octave below the note of the fork. Its second harmonic, which is always an octave higher than the fundamental note, will now correspond to the note of the fork. Hence the cord still responds by breaking up into two segments, with a node in the middle. Change now the weight to one ounce and three-fourths. This is about one-ninth of the original weight. Hence the cord will vibrate only once while the fork makes three vibrations, so that there is a difference of twelve notes between them. If the note of the cord were C, that of the fork would be G in the next higher octave. Now it happens that the third harmonic of a cord is always twelve notes higher than its fundamental tone; therefore, the fork may call out a response from the cord, which divides into three segments, and

gives forth *visibly* its third harmonic. In a similar way, a tension of one ounce makes the string divide into four segments, by reducing its fundamental note by two octaves. A tension of one quarter of an ounce makes the string divide into eight segments, by reducing its fundamental note by three octaves. A heavier cord is also sometimes used, requiring a weight of one hundred and forty-four ounces when it vibrates in one segment, which makes the phenomena still more conspicuous. With either cord, if the tension is not such as to bring some one of the harmonics into unison with the tuning-fork, the cord is *visibly* silent.

These experiments are particularly valuable as illustrating two propositions.

1. They furnish an excellent exemplification of the great mechanical results which originate in sympathetic motions, when bodies, which stand in each others neighborhood, are nicely attuned to one another in their rates of vibration. Bridges are endangered by the passage of troops when marching to the regular beat of music, and buildings are weakened or destroyed sometimes by the *well-timed* motion of the machinery. It is well known in the factories of Lowell, that the walls will vibrate when the machinery goes at a fixed rate; but if the speed is increased or slackened, they stand perfectly still. In one instance, water was nearly emptied out of a pail by the agitation of the floor on which it stood; bnt when the cause was discovered the remedy was a simple one, viz.: to alter the rate at which the machinery moved.

2. These experiments are valuable, in the second place, as illustrating the *optical* method of studying the laws of Acoustics. In this view of the subject, the paper now presented and the experiments which accompany it, may be considered as a continuation of the paper and the experiments which I offered at the last meeting of the Association. At the Burlington meeting, I discussed the optical method of studying the laws of vibration of *columns of air*. The advantage of the *optical* method of studying *sound* is great, as it makes the experiment independent of the musical excellence of the ear of the experimenter, and relieves it of a large *personal equation* in the observer. Any person, by the use of this method, may accom-

plish more, with the *coarsest vision*, than would be possible, by the ordinary methods, to the most highly cultivated musical *ear*.

---

2. Laws of the Ocean Currents. By J. S. Grimes of New York.

PRELIMINARY EXPLANATIONS.

1. The prime moving cause of all the constant ocean currents is the difference of temperature between the warmer and colder latitudes. The surface water in the colder latitudes becomes cool and condensed so as to sink and flow in deeper currents towards the equator, while the surface water in the tropical latitudes is warmed and overflows towards the poles. These two currents continually balance and compensate each other. Were it not that the earth turns daily on its axis from west to east, no farther explanation of the ocean currents would be required.

2. It is now well known to geographers that when a current flows from the equator the rotation of the globe causes it to flow easterly, that is it flows north-easterly in the northern and south-easterly in the southern hemisphere. When on the contrary a current flows towards the equator the effect of rotation is to deflect it in a westerly direction — that is south-west in the northern and north-west in the southern hemisphere. This also is very simple, and is a sufficient explanation of all the currents of the ocean excepting five. There are five great elliptical currents, one in each of the oceans, that require farther explanation.

3. There are then two classes of ocean currents, the local and the elliptical. The local currents I have already described. They move either north or south and do not return, but are lost in the masses of the ocean. The elliptical currents have not hitherto been understood; indeed, it is only within a few years that their very existence as a class has been recognized. According to the best information that I have been able to obtain, Professor Joseph Henry, of Washington, was the first to announce the fact that in each of the five great oceans there is one vast whirl, or elliptical current. His article was pub-

lished in one of the patent office reports about ten years ago. Professor Dana in his excellent Manual of Geology not only makes the same statement, but illustrates it by an engraving.

The only hypothesis that has been offered to account for these whirls or ellipses, is that they are owing to the basin-like forms of the depressions in the earth's crust in which the ocean is contained. If we do not scrutinize this hypothesis too closely, it seems quite plausible. The warm currents move north-east (in this hemisphere) and the cold currents south-west, and both being forced against the sides of the basin, are driven entirely around it. I say this seems plausible; but when we apply the *experimentum crucis* the hypothesis fails, for in repeated instances the currents leave the shores or sides of the basins and flow off in tangents to pursue their own elliptical orbits; or, if like planets, they are impelled by an invisible and irresistable power, over which neither the shores nor the winds have any essential influence. Observe, for instance, the elliptical current of the North Pacific, leaving the coast of California to flow south-west, while the coast trends to the south-east. See the same current in mid-ocean before it reaches Asia turning north-west. Here is no land nor basin sides to deflect the current.

Again, in the South Pacific, the current flows from the Antartic Sea north-east to Chili. Afterwards it leaves the coast of Peru and flows south-west, and then west, and while flowing in mid-ocean it turns again and flows south between Australia and New Zealand. In the South Atlantic observe the current flow from the coast of Brazil eastward across the ocean to Africa. In the Indian Ocean, observe how the elliptical current keeps on the south side of the equator, and avoids the islands and the coast of southern Asia. It is plain then that this hypothesis, when brought face to face with facts, is entirely discredited.

Taking for my guidance the most authentic maps and charts that I could obtain, I have found that an elliptical current does not in any instance extend much farther from the equator than the forty-fifth parallel. I do not think that in any case it reaches as far as the fiftieth parallel, though, of course, the change of the seasons produces some variations. In all five of

the oceans, very near the forty-fifth parallel, the current flows due east nearly across the ocean to which the current belongs.

I find, also, that in each ocean, on the western side, there is a point where the current changes its course from westerly and begins to move easterly, and in all cases this point is very near the twenty-fifth parallel. On the eastern side of the ocean there is an analogous turning point, but it is near the thirty-fifth parallel, that is, it is ten degrees farther from the equator than the turning point on the western side of the ocean. These turning points, at some seasons, vary somewhat from the parallels I have assigned to them, but in every case it will be found that the eastern turning point is about ten degrees north of the western.

A true theory should explain these points and limitations of the currents, and give good reasons for every one of their peculiarities.

ELLIPTICAL CURRENTS.

Let us take the North Atlantic current as an example by means of which to illustrate the laws that govern all elliptical currents. This current may, for convenience, be said to commence in the Gulf of Mexico, at the twenty-fifth degree of north latitude. Here the water of the current is neutral, that is, it has the same easterly velocity that the globe in the same latitude has. From the Gulf the current flows north-east. It flows north because it is warm, and east because it carries with it more easterly force or easting than is possessed by the globe in more northern latitudes. The farther north it flows the more easting it acquires, and the more it differs from the places in which it arrives. When it reaches the forty-fifth degree of north latitude at the Grand Banks of Newfoundland it has acquired so much easting that it flows due east nearly across the ocean.

This will not appear incredible when we consider that the transferred water left the Gulf with an easterly velocity of two hundred and five miles per hour greater than is possessed by the water on the Grand Banks. The wonder might rather be that the current did not flow due east before it reached as far north as the Grand Banks.

The question may occur why the water of the ocean current

only moves about two miles an hour. If the water of the Gulf could by some means be instantly transferred from the twenty-fifth to the forty-fifth parallel, it would move eastward two hundred and five miles per hour. The reason why the current only moves two miles instead of two hundred is that the quantity of the water transferred from the Gulf to the Banks is small compared with the great mass of ocean, and the force of a particle of the transferred water is diffused and divided among a great many particles, so that the current gains in mass what it loses in velocity.

Many authors, finding it necessary to give some reason for the current turning east so suddenly at the Grand Banks, assert that it is deflected east by the Banks themselves. But when we consider the great difference in easterly velocity (205 miles per hour), between the twenty-fifth and forty-fifth parallels, we need be at no loss for the reason why the current turns from north-east to due east.

Before the current reaches the shores of Europe it turns to the south of east; this also has been vaguely accounted for by saying that it is deflected by the shores. But the truth is, that it has become cooled in crossing the ocean. The warm water at the surface cooled first and sank and turned southerly; its place was immediately occupied by the warmer particles below, and these in turn cooled, sunk and turned south in the same manner. So much of the current as did not thus become cooled turned to the north-east, and flowed to the Arctic ocean; but the cooled portion flowed south, as all cold ocean water in the northern hemisphere does.

At the thirty-fifth parallel the current ceases to flow easterly, and turns to the south-west. The reason of this change is that the surplus easterly force is exhausted. Here, therefore, the water becomes neutral. It has just the same easterly velocity that the globe in that latitude has, and no more. Let me here remark, that in each of the five great ellipses there are two neutral points where the current and the globe have the same easterly velocity; one of these is at the twenty-fifth and the other at the thirty-fifth parallel. I doubt whether either of these points ever varies five degrees from those parallels.

It will be observed that the current begins to flow easterly

at the twenty-fifth parallel and that it does not reach the same parallel again before it ceases to move easterly. It falls short ten degrees, and ceases to flow easterly at the thirty-fifth parallel. If the current could move, as a planet does, without expending any of its force, it would reach the twenty-fifth parallel before its easterly force would be exhausted, but the current expends so much of the easterly force that it carries with it from the twenty-fifth parallel, that it is exhausted when it arrives at the thirty-fifth parallel. It seems, therefore, that the easterly force expended by the current on its journey from the twenty-fifth parallel to the thirty-fifth, is equal to the force required to continue it in an easterly direction ten degrees farther.

When the cold neutral current flows south of the thirty-fifth parallel, it immediately enters latitudes that possess more and more easterly velocity than the current does. Of course the current flows relatively south-west. The farther south the current flows, the more westerly it flows, until, near the equator, it is impelled in a due west direction by an irresistible force.

I have already explained that when the current flows due east, it continually cools, and turns southerly; but now, while flowing due west near the equator, it becomes warmer and warmer, and the heated surface water turns northward, as all warm current do in the northern hemisphere. The current, therefore, flows north-west to the Gulf of Mexico, whence it started. Here its westerly force is exhausted, it becomes neutral again, and is ready to repeat its elliptical circuit.

It is an interesting, as well as important fact, that the laws which govern the currents are such as to limit the extent of the ellipses, and fix their boundaries in all directions.

In the Atlantic the ellipses have not quite room enough to develop themselves to their full extent, but in the Pacific there is evidently "room and verge enough" to enable them to expand, and exhibit their proper normal dimensions. At the first thought it might seem, that, if North America were to sink, the Atlantic and Pacific being one great ocean, there would be but one vast elliptical current, where now there are two; but a more rigid examination will result in the conviction that the two currents would still continue to flow in separate

orbits, and perfectly independent of each other. Let us once more briefly review the ellipse and show that the path of a current is fixed by laws as definite and inexorable as those that determine the paths of the planets in their orbits.

When a current flows north-east from the twenty-fifth parallel, it continues to accumulate more and more easting, until it reaches the forty-fifth parallel, when its easterly force is so great that it is compelled to flow due east. The extent of the ellipse in a northern direction, is therefore limited to the forty-fifth degree.

When the current reaches the thirty-fifth parallel, the easting is exhausted, and therefor the farther extension of the ellipse in that direction is impossible. When the current flows near to the equator it has accumulated so much westing that it is forced to flow due west; it cannot cross the equator, and the extent of the ellipse is thus limited. When the current flows north-west from the equator it reaches the twenty-fifth parallel, and then the westing being exhausted the ellipse cannot be extended any farther west. It seems that a current cannot flow alternately north and south between the equater and the forty-fifth parallel without assuming an elliptical or rather an ellipsoidal both essentially like that in the diagram.

In regard to Local currents, they never flow within the limits of an elliptical circuit, but are generated outside of them by excessive cold or heat in situations where the water cannot restore its equilibrium by means of any one of the great elliptical currents.

It may be said that all currents tend to flow in elliptical circuits, but local currents are so situated that they have not room enough to make a circuit and return. They therefore flow in curvilinear paths until they are lost and merged in the great ocean. On the *eastern* side of any ocean a *warm* current is always local and flows counter to the course of the elliptical current. On the *western* side a *cold* current is always local and flows counter to the course of the elliptical current. Our local current never flows counter to another in the same latitude and on the same side of the ocean.

The most remarkable of all the local currents, if indeed it can be properly called local, is the Atlantico-Arctic current.

It flows north-east from the middle of the Atlantic in the forty-fifth degree of north latitude, and passing through the British Archipelago, it flows along the coast of Norway into the Arctic ocean. By this time it has acquired so much easting that it flows nearly due east half way around the pole, and then escapes along the east coast of Greenland into the Atlantic. Acquiring more and more westing as it flows more southerly, it hugs the United States coast and proceeds as a deep cold current into the Caribbean Sea. Its *tendency* now is to escape west into the Pacific, but the American continent prevents this and it remains until it becomes warm, and then turns and retraces its course north-easterly in company with the regular elliptical current, until it reaches the forty-fifth degree, and then repeats its circuit.

The most noted local current in the south Atlantic is called the Guinea current. It appears to be generated near the equator, between the two ellipses, and flows south and south-east into the Gulf of Guinea, between the elliptical current and the African coast. It flows parallel with the elliptical current but in the opposite direction. Malte-Brun say that "no adequate cause can be given for this current." I can readily understand that in the absence of the principles explained in this essay, not only Malte-Brun but every other geographer would be unable to assign an adequate cause for two large currents flowing side by side, but in opposite directions.

The Cape Horn current in the North Pacific, is perfectly analogous. It is a warm current generated on the West Coast of South America, which of course flows south and east along the coast and escapes into the Atlantic around Cape Horn. It is described, I believe, by all writers, as a part or branch of the great cold current (the elliptical current) that flows from the Antarctic ocean to Peru. But the truth is that it is a perfectly independent warm current, flowing south and east, while the great Peruvian current is cold and flows north and west. In the North Pacific, near the equator and between the two great ellipses, is a counter local warm current which flows east to the American coast, and then north between the coast and the ellipse, as far as Oregon, and perhaps farther. I have been informed by Commodore Hitchcock of the American Navy, that

he has observed a similar current in a corresponding situation in the Atlantic. I presume that the current which he observed is a part of the so called Guinea current. I regard the Guinea current, the Cape Horn current, and the North Pacific counter current, as all analogous to each other.

There are some so called periodical currents in the land-locked seas of Arabia, Bengal, China and Australia, which are supposed to be produced by the monsoon, or season winds. But there is a lack of correct information concerning them. I suspect, however, that the winds have but little agency in producing these or any other important and deep currents. It is more likely that the same changes of the season which produce the monsoon, or season winds, are also the real causes of the periodical, or season currents of the sea.

---

3. THE NATURE OF ELECTRIC DISCHARGE. By Prof. O. N. STODDARD, of Oxford, Ohio.

THAT theory seems very unwieldy which conceives of electricity as two fluids imponderable and indefinitely rare; quiet when united but intensely active when separated, and that all the violent effects are due to the transfer of the fluids through bodies. And this conception constantly manifests itself in the statements of the laws and phenomena of electricity.

Few, it is believed, will object at present to the view that the electric force is molecular; that whatever doubt may exist as to its precise nature and mode of operation, that it is connected with and acts by and through those particles which chemistry contemplates as atomic; that whatever changes or disruptions are wrought by it, are operations going on among these atoms; and that electric discharge is not the transfer of matter, but of some change along the line of discharge, producing other correlated forces. All electrical excitement preceding discharge is a state of induction, transferred, according to Faraday, along lines of particles to other bodies. This condition is one of tension, and the tendency of the particles is to undergo some change antagonistic to cohesion, and consequently resisted by this force. As long as cohesion can

resist the electric force, the condition is statical. If the tension rises to a sufficient degree, the particles yield and undergo the change which the force requires. This is discharge. The change, in which consists discharge, will be violent in proportion to the degree of tension, and the character of the change will depend upon the nature of the body. The substance may be split—torn asunder—as in trees; ground to powder or fractured, as in glass and other brittle bodies; melted as in metals, or chemically separated as in compound bodies. These various actions are the equivalents of the electric force, and are its correlated forms.

The energy, in that particular form we call electricity, has in each of these cases of discharge been expended. As a cause it has acted, and as such has disappeared. Its farther existence must be sought for in its effects. The mechanical and chemical separations, the heat and the light, are all so many evidences of the electric force, and exact measures of it.

That all these changes are the results of molecular displacements seems plain from the effects. When passed through a thick plate of glass the discharge pulverizes along its track the glass to an impalpable powder. A violent separation of particle from particle has taken place, as if the molecular interstices were filled with an explosive substance.

A blow from a hard body, or from a fluid shot with enormous velocity, against the glass, would merely drive the particles before it and leave a clear opening. But in electric discharges each particle is so disrupted from the adjacent ones as to break up the cohesion, while none are thrown off except at the surface, where there is no force to counteract the explosive action. The pulverized atoms must occupy more space on account of diminished density, and naturally press outwards in the direction of least resistance. The burr on both sides of a card merely indicates the strong molecular repulsion acting from within outward. The imprisoned air in bodies of loose texture will have its influence, being itself subject to the same disruption, but the double burr will be produced in thin plates of wax, in which air cannot be present in sensible quantity. When a tree is riven by lightning the splintering is caused by the repulsive energy acting along the line of discharge. If

the fibres of the wood, where the fracture takes place, had been changed to gunpowder, or gun-cotton, and exploded, the results would closely represent those effected by electricity.

So when a body is melted by the discharge, the heat in producing liquidity certainly acts upon the molecules. But it is unreasonable to suppose that the heat is developed by the discharge merely on the surface, and then passed inward by the slow process of conduction. On the contrary, it flashes into intense energy, at the same instant, among all the particles of the mass. The inconceivable rapidity of the change from a solid to a liquid, caused by electricity, leaves no time for conduction.

That chemical affinity is a molecular force no one, it is presumed, will doubt. But the very intimate relations of the chemical and electric forces — so intimate as to induce in some minds a belief of their identity — will hardly permit us to assign a molecular character to one and not to the other. The light which pervades the atmosphere for some distance around the point where lightning strikes the earth, does not seem, from the cases the writer has had the dangerous pleasure of witnessing, to be a gleam of reflection from the brighter track of the spark, but rather a light produced in the air, for some distance around, by molecular disturbance; the same in kind, but less in degree, as that which rivals sunlight along the central path. A green tree, at which the writer happened to be looking the moment it was struck, was covered from top to bottom with a diffused flame, in the midst of which the central stream of fire gleamed in tortuous course along the trunk. It is not to be supposed, however, that the electric induction, and the tension caused by it, which precedes discharge, exist only along the line of violent action.

The line of discharge may be considered the *resultant* of the electric tension for some distance around, and along this line is expended at the moment, all the accumulated force from inductive action. The violently disruptive effects which sometimes occur, are adequately explained by the heat produced along the line of discharge.

No other known agency generates heat so suddenly and of such intensity as the electric force. The reason is plain. The

elctric force being itself molecular, developes, or as some would prefer, is *converted* into heat at the same instant throughout the whole body, and its repulsive power is exerted simultaneously between all the particles, and needs no transfer by conduction. The violence with which some chemical compounds explode, is proportional, other things being equal, to the consentaneousness of the action among the particles. Allowing the velocity of electricity to be the same as that of light, the time in which a coin or metallic button on the person, would be melted is inconceivably short, less than the one *twelve billionth* of a *second*. The same explosive action expended on the air causes the terrific sound of thunder. Near the path of discharge, it is a crash, sharp, spiteful. At a distance it is toned down by the elasticity of the air into a roar which shakes the very earth.

It seems strange that some writers should attribute the sound to the *collapse* of the air after displacement. The collapse is but the reaction from the condensed air around the line of displacement, and of course cannot exceed the force of displacement. The latter, then, is the primary cause of the sound; the collapse only repeats the undulation with some loss of force.

Surprise is sometimes expressed that the human body when struck is not torn. The electricity is a good deal diffused through so large and so good a conductor, but the texture of the body is of such a nature — composed of porous and elastic solids filled with liquids — that its particles readily yield to the requirements of the force, without disruption.

The mechanical and perhaps chemical disturbances of the nervous system — even if there be no other — are quite sufficient to account for the fatal effects of the electric shock. If it is allowable in a scientific article to introduce one's own experience of an electric shock more violent than one usually takes, then the writer would say, that the effects of a discharge from a battery of twelve jars, one gallon each, passed through the length of the body, is best expressed by the simple phrase — *a stunning blow*.

If the general laws of electric discharge, which I have stated, are correct, then our precautions for safety should con-

form to them. A lightning rod is intended to afford a ready passage between the cloud and earth, to any electricity which may be accumulating. If it discharges the electricity as fast as it is generated, then there can be no collection in quantity or intensity sufficient to cause violence.

The rod is a good conductor; that is, its particles readily assume the conditions required for discharge; hence it offers a comparatively unobstructed channel for the passage of the electricity, and of course prevents any dangerous accumulation. The rod takes time by the forelock—it anticipates the danger. It does not allow this fierce agent to *store up* its power, but gradually and safely transmits to earth an amount of electric energy, which, if discharged at once, would have manifested intense violence. If the powder in a cannon were burned grain by grain, during a few seconds, it would not even move the ball; and yet the same force has been expended, as, when exploded at once, it hurls the missile with deadly force. Of late I have been inclined to consider the electricity of the cloud to bear to that of the earth the relation of *intensity* to *quantity*. That an equivalency of force may exist between intensity and quantity is acknowledged by all. Quantity in the galvanic battery has its equivalent force in the intense flash of the induction coil. It may also be admitted that quantity in the earth is represented by intensity on the points of the rod. In this case an identity of action is established between the points and the cloud. It hardly need be said that to insulate a rod from the building it is intended to protect is useless, if not worse. The *house* is in a state of electric tension as well as the rod, and the object of the rod is to relieve all such tension. To do this the rod must be in electric communication with the house. Indeed the insulation is after all a sheer pretense, and is practically impossible; for the building and rod *are* connected through *the earth*. If the roof is metallic all the better. It affords surface to diffuse the force, sharp edges and innumerable points to discharge it. Erect the rods then upon the roof.

No better conductors can be constructed to connect such a roof with the earth than its water pipes. Pass iron or copper rods from these pipes into moist earth; or better still, connect

one or more of them with the water-pipes of a city, or with the water of a well; or in lieu of these, with the gas pipes, and insurance of such buildings against damage would be profitable to a company at exceedingly low rates.

---

4. INFLUENCE OF THE MOON UPON THE WEATHER. By Prof. ELIAS LOOMIS, of Yale College.

SEVERAL meteorologists have attempted, by a comparison of a long series of observations, to determine whether the Moon exerts any influence upon the weather. From a comparison of twenty eight years of observations in Germany, Schübler, in 1830, deduced a sensible influence of the moon, the number of rainy days at the time of the second octant being 25 per cent. greater than at the time of the fourth octant. From a comparison of observations made at Paris, Orange and Carlsruhe, Gasparin arrived at results not differing greatly from those of Schübler.

By a comparison of sixteen years of observations at Greenwich, nine years at Oxford, and sixteen years at Berlin, Mr. Harrison, of England (Ast. Soc. Month. Not., v. 28, p. 39), has obtained results which are remarkably consistent with each other, and which indicate that the moon exerts an appreciable influence upon terrestrial temperature, the maximum occurring six or seven days after new moon, and the minimum about four days after full. The difference between the maximum near the first quarter, and the minimum near the last quarter, is two and a half degrees of Fahrenheit. These results which are so different from what might have been anticipated, Mr. Harrison explains by supposing that the moon really attains its greatest heat about the last quarter; but that the heat which the moon radiates to the earth is entirely dark heat, and therefore absorbed by our atmosphere. This heat raises the temperature of the air above the clouds, causing increased evaporation from their surface, by which they are dispersed; and thus there is an increased radiation of terrestrial heat to the sky, and consequently a diminution in the temperature of

the air near the ground. He supposes that opposite results must occur at the period of minimum heat in the moon.

Upon extending the comparison to forty three years of observations at Greenwich (Rep. Br. Asso., 1859, p. 193), Mr. Harrison still finds a fluctuation of temperature, but the range is reduced to 1°.1 instead of 2°.5.

Buys Ballot on tabulating a series of seventy years mean daily temperatures, according to the moon's age, found that the highest temperature occurred during the seven days after full moon; being almost precisely opposite to the results of Mr. Harrison.

Schiaparelli (Mem. del R. Ist Lomb., v. x,) has made a careful analysis of thirty eight years of observations made at Vigivano, near Milan in Northern Italy, and has obtained results which are also remarkably consistent with each other. They show that about the time of the last quarter of the moon, there is a maximum in the number of rainy days, as also in the frequency of storms and in the degree of cloudiness.

At the meeting of the American Association in 1853, I presented the results of seven years observations of the amount of cloudiness at Greenwich. The average results for each day of the moon's age ranged from 6.1 to 7.0; but the fluctuations did not follow any obvious law. If, however, we eliminate the influence of accidental causes, by substituting for each day's result the average of five days (including the two preceding and two following days), the numbers thus obtained are remarkably consistent with each other, as shown in the following table, where the degree of cloudiness is expressed in tenths of the entire sky.

AVERAGE CLOUDINESS.		AVERAGE CLOUDINESS.		AVERAGE CLOUDINESS.		AVERAGE CLOUDINESS.	
3 days bef. new,	6.48	3 days bef. 1st qr.,	6.58	3 days bef. full,	6.78	3 days bef. 3d qr.,	6.76
2 " " "	6.54	2 " " "	6.60	2 " " "	6.74	2 " " "	6.92
1 day " "	6.60	1 day " "	6.60	1 day " "	6.76	1 day " "	6.86
New moon,	6.60	First quarter,	6.64	Full moon,	6.68	Third quarter,	6.76
1 day after new,	6.60	1 day after 1st qr.,	6.66	1 day after full,	6.70	1 day after 3d qr.,	6.64
2 days " "	6.68	2 days " "	6.68	2 days " "	6.70	2 days " "	6.58
3 " " "	6.62	3 " " "	6.70	3 " " "	6.72	3 " " "	6.52

The general character of these results will be best perceived from the curve line, fig. 4, on Plate I. Above this curve I have placed the curve, fig. 3, showing the number of rainy days for

each day of the moon, according to the results of Schiaparelli. The general agreement of the two curves is quite remarkable, and seems to indicate a real influence of the moon upon the clouds, the cloudiness being greatest about the time of last quarter.

In order to compare the influence of the moon with that of the sun, I have taken the average cloudiness at Greenwich as indicated by observations made every two hours for a period of seven years. The following table shows the result, the hours being expressed in Göttingen mean time.

DATE.	CLOUDINESS.	DATE.	CLOUDINESS.	DATE.	CLOUDINESS.	DATE.	CLOUDINESS.
0 h	7.14	6 h	6.67	12 h	6.90	18 h	6.90
2	7.14	8	6.29	14	6.49	20	6.97
4	7.03	10	6.06	16	6.80	22	7.14

The difference between the greatest and least cloudiness is 1.06, which may be regarded as a measure of the sun's influence. The moon's influence, as shown in the preceding table, is 0.44, indicating that the sun's influence is two and a half times that of the moon.

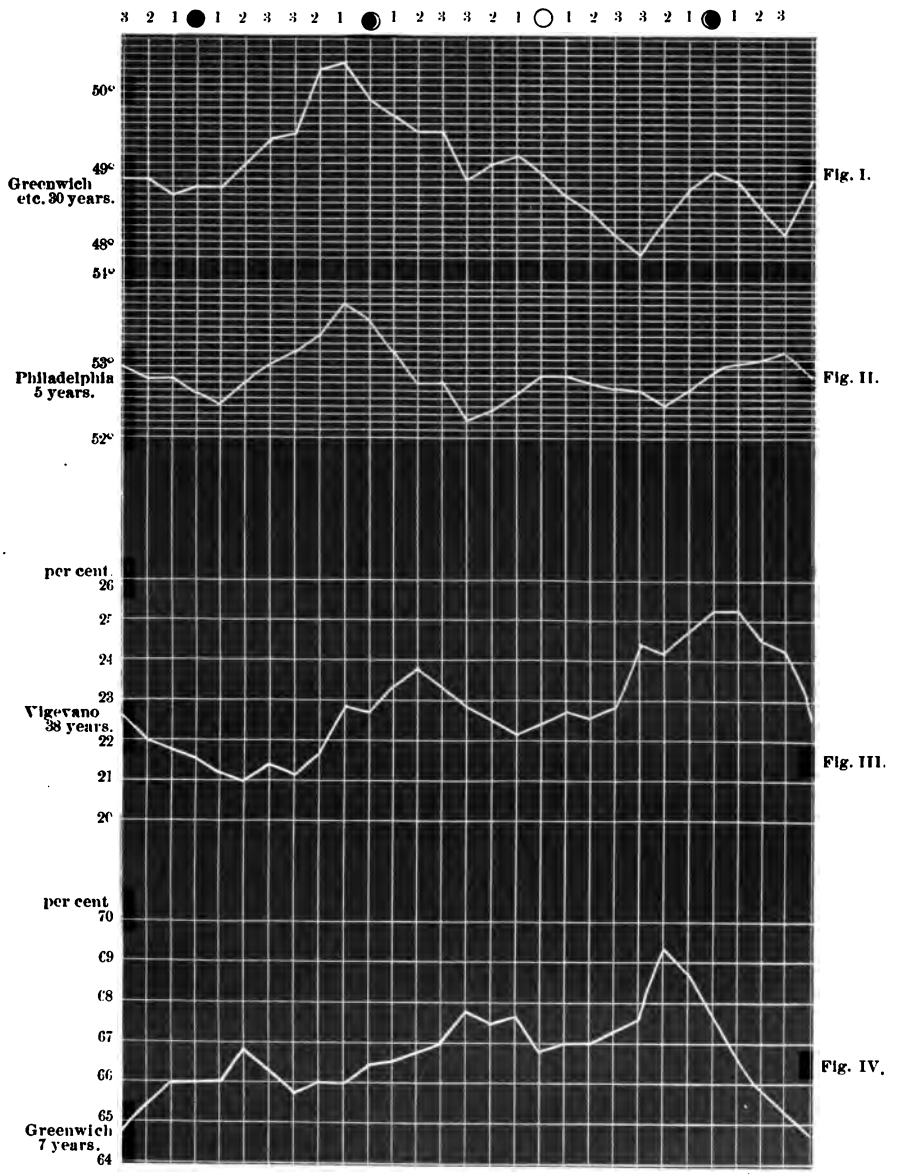
In order to determine whether the moon exerts any appreciable influence upon terrestrial temperature, I have arranged the observations made at Girard College from 1840 to 1845, according to the phases of the moon, and taken the average for each day of the moon's age, as shown upon the accompanying sheet. If now, in order to eliminate the influence of accidental causes, we substitute for each day's result the average of five days (including the two preceding and two following days), the numbers thus obtained exhibit a remarkable consistency, as shown in the following table.

AVERAGE TEMPERATURE.	AVERAGE TEMPERATURE.	AVERAGE TEMPERATURE.	AVERAGE TEMPERATURE.
3 days bef. new, 52°.83	3 days bef. 1st qr. 53.12	3 days bef. full, 52.25	3 days bef. 3d qr. 52.62
2 " " " 52.74	2 " " " 53.31	2 " " " 52.36	2 " " " 52.46
1 day " " 52.75	1 day " " 53.68	1 day " " 52.58	1 day " " 52.63
New moon, 52.56	First quarter, 53.48	Full moon, 52.82	Third quarter, 52.89
1 day after new, 52.46	1 day aft. 1st qr. 53.06	1 day after full, 52.82	1 day after 3d qr. 52.97
2 days " " 52.76	2 days " " 52.71	2 days " " 52.70	2 days " " 53.07
3 " " " 52.97	3 " " " 52.67	3 " " " 52.64	3 " " " 53.14

The general character of these results will be best perceived from the curve line, fig. 2, on Plate I. The range of the fluctuations is considerably reduced by taking the mean of five

OB

Date.	3
1840.	
	66
	71
	73
	44
	51
	31
1841.	1
	2
	4
	6
	6
	7
	7
	5
	3
	4
1842.	3
	4
	4
	5
	5
	7
	6
	6
1843.	5
	5
	1
	1
1844.	
1845.	
Means.	



CURVES OF MEAN TEMPERATURE AND CLOUDINESS ACCORDING TO THE AGE OF THE MOON.

successive days; but the characteristic features of the curve are still preserved. Above this curve I have placed curve line, fig. 1, representing the mean result of thirty years' observations at Greenwich and Berlin. It will be perceived that both series of observations indicate a maximum temperature a day or two before the first quarter, and the principal undulations of the two curves exhibit a similarity so decided as to afford a strong presumption that they are mainly due to lunar influence. The explanation assigned by Mr. Harrison is, however, contradicted by the observations represented in fig. 4, which shows that the cloudiness of the sky is greatest about the time when the heat of the moon should be the greatest.

The mean diurnal change of temperature at Greenwich and Berlin is a little over ten degrees, which represents the average diurnal effect of the sun's rays at these places. The monthly fluctuation of temperature, which is apparently due to the moon's influence, is somewhat over two degrees; or the influence of the sun is about five times that of the moon. Nearly the same ratio results from the observations at Philadelphia.

In 1853, Althaus (Pog. An., v. 166, p. 544) undertook, by improved methods, to measure exactly the amount of the solar radiation; and he applied his method to determine the effect produced upon the moon's surface. His conclusions were that the temperature of the moon's surface fluctuates from considerably below zero to several hundred degrees above zero, and attains its maximum about seven days after full. This last conclusion coincides with the results deduced by Buys Ballot from seventy years' observations in Holland (Pog. An., v. 146, p. 163), but seems at variance with the observations made at Greenwich and Philadelphia.

The effect of the moon upon the cloudiness of the sky appears to be decided, and to follow nearly the same law as the influence of the sun. The mean degree of cloudiness is greatest about noon, and least about nine o'clock in the evening; that is, the maximum of cloudiness slightly precedes the hour of greatest heat, and the minimum follows the maximum at an interval about equal to one third the time of the sun's revolution. So, also, the greatest cloudiness due to the moon's influence occurs about the time of greatest heat; that is, near the

third quarter; and the minimum follows about a week after the maximum, or about one fourth the time of the moon's revolution.

This disturbance of the vapor of our atmosphere gives rise to storms, during which vapor is precipitated, and a vast amount of latent heat is liberated. The motion of these storms over the earth's surface is influenced by local causes, such as geographical latitude, proximity to mountains or the ocean, and hence the effect of these storms upon terrestrial temperatures may not be everywhere the same. This principle may, perhaps, explain the apparently opposite effects of the moon upon the temperature of different localities.

---

5. ON THE RELATIVE VALUE OF GOLD AND SILVER FOR A SERIES OF YEARS.* By E. B. ELLIOTT, of Washington, D. C.

(ABSTRACT.)

THE speaker began with an account of the relative standard value of gold and silver adopted by different nations in their coinage. His object was a practical one, bearing on the recommendations of the commission on a uniform system of coinage lately in session in Paris, in connection with the Exposition. Formerly the French adopted 1 to 15½ for the ratio of the value of silver to gold in their coinage. After the influx of gold from California and Australia, the market value of gold relative to silver fell below this ratio, and the inevitable effect was that the silver coin was remelted or exported nearly as fast as coined.

To obviate this evil, the silver coins smaller than the five-franc piece were debased, in accordance with a recommendation of a quadripartite convention concluded in 1865, between France, Belgium, Italy and Switzerland. This debasement Mr. E. conceives to be a mistake. They ought instead to have increased the amount of gold in the gold coinage about 3½ per cent., thereby conforming to the simpler, and in every respect preferable ratio of 1 to 15. There would then have been no

*This paper was read at Burlington, and was accidentally omitted in the printed volume of Burlington Proceedings.

need of changing the fineness of their smaller silver coinage from the simple decimal ratio, nine-tenths which had previously been declared "immutable," to the more complicated one of 835 thousandths. Then the new gold five-franc piece would have weighed one-third of five grammes, a very simple ratio, and would have been *almost the exact value of the American gold dollar.* The system of the United States is almost strictly metrical. The new French system (quadripartite convention) is unmetrical, having complicated relations to their beautiful system of weights and measures. Yet the international commission has recommended the adoption of the latter, and thereby the perpetuation of what originally was simply a mistake. Whereas, had a gold coin less only by one third of one per cent. than the American gold dollar been adopted as the standard, the French silver coinage could be maintained at its old standard fineness of nine tenths, and would continue in entire harmony not only with the metric system, but also with the present and past market values of the metal; while the American coinage would not require appreciable change.

The paper was illustrated by a diagram.

---

6. A NEW FORMULA FOR THE REDUCTION OF OBSERVATIONS IN THE PRIME VERTICAL, ANALOGOUS TO MAYER'S FORMULA FOR THE REDUCTION OF MERIDIAN OBSERVATIONS. By WILLIAM A. ROGERS, of Alfred Center, N. Y.

IT is easily proved:—

I. That, if from any point of the celestial sphere as a pole, great circles be drawn through two points nearly equidistant from the assumed pole, then, for any small arc, the distance between the two points will be equal to the angle at the pole multiplied by the sine of the polar distance.

II. That the time required to describe a small diurnal arc near the meridian, is equal to the given arc divided by the cosine of its declination.

III. That the time required to describe a small diurnal arc near the prime vertical is equal to the given arc divided

by the product of the sines of the latitude and the zenith distance.

Let, a. b. c.

equal the azimuth, level and collimation constants. Then, by I, for any zenith distance these functions become:—

$a \sin n$. $b \cos n$. c.

For either the meridian or the prime vertical,

The time of transit over the given threads = the observed time.
+ the clock error.
+ the time of describing the arc $a \sin \phi$.
+ the time of describing the arc $b \cos \phi$.
+ the time of describing the arc c.

For the meridian, by II,

$$T = T' + \Delta T' + \frac{a \sin z}{\cos \delta} + \frac{b \cos z}{\cos \delta} + \frac{c}{\cos \delta}$$

which is Mayer's formula.

For the prime vertical, by III,

$$T = T' + \Delta T' + \frac{a}{\sin \phi} + \frac{b}{\sin \phi \tan z} + \frac{c}{\sin \phi \sin z}$$

which is the form given by Buïnoso.

But this equation is true only for a single thread, whose distance from the line of sight is a constant quantity c. Since, during the passage of a star over several threads, the zenith distance undergoes a continual change of value, the mean of the observed times on the several threads will not coincide with the time on the mean of the threads. In order, therefore, to apply the preceding formula, it is necessary to reduce the time on each thread to the time on the mean of the threads by the usual formula.

$$I = \frac{i}{\sin \phi \cos \delta \sin (\theta' - T)}$$

an operation which is exceedingly tedious, and likewise requires a knowledge of the thread intervals.

I have found that the reduction from the mean of the observed times to the mean of the threads, can be completely effected by the simple expression,

$$\theta - \theta' = [.0031681^s] \text{ [no. units in log. k] } \cot \theta'$$

where,

θ = the hour angle for the mean of the threads.
θ' = the hour angle for the mean of the observed times.
log. k = a function of the variable hour angle, and is to be taken from Table VIII, Chauvenet's Astronomy, Vol. II.

The formula for the mean of the threads therefore becomes:

$$T = T' + \Delta T' + [.0031681^s]\ [\text{no. units in log. k}]$$

$$\theta + \frac{a}{\sin\phi} + \frac{b}{\sin\phi \tan z} + \frac{c}{\sin\phi \sin z}$$

The following reduction is given as a test of the accuracy of the formula and also to show its superiority over the ordinary form both in brevity and simplicity.

By the ordinary form:—

CONSTANTS.

$\theta' = 4^\circ\ 33'\ 24''$ log. sin ϕ cos δ = 969,761.

T′ I ½I $\theta - \frac{1}{2}$I log. sin ($\theta - \frac{1}{2}$I) log. sin ϕ cos δ sin ($\theta - \frac{1}{2}$I) log. i log. I I T.

Th'ds.	h.	m.	s.	m.	s.	′	″	°	′	″					m.	s.	°	′	″
1	9	50	51.0	+5	22.8	+40	21	−5	13	45	8.96971	8.65732	1.13033	2.47310	+4	57.2	9	55	48.2
2	9	51	52.0	+4	21.8	+32	43	−5	6	7	8.94904 n	8.64665 n	1.02325 n	2.37660	+3	58.0			50.0
3	9	52	54.2	+3	19.6	+24	57	−4	58	21	8.93791 n	8.63552 n	.87564 n	2.24012	+2	53.8			48.0
4	9	55	15.2	+0	58.6	+ 7	19	−4	40	43	8.91151 n	8.60912 n	.11797 n	1.50815	+0	32.2			47.9
5	9	56	29.2	−0	15.4	− 1	55	−4	31	29	8.89702 n	8.59463 n	.19312 n	.59849	−0	39.7			49.5
6	9	59	9.2	−2	55.4	−21	55	−4	11	29	8.86385 n	8.56146 n	.87040 n	2.30894	−3	28.7			45.5
7	10	0	48.2	−4	34.4	−34	18	−3	59	06	8.84195 n	8.53956 n	1.01995 n	2.48089	−5	2.3			45.9
8	10	2	31.6	−6	17.8	−47	15	−3	46	11	8.81787 n	8.51548 n	1.12743 n	2.61195	−6	49.2			42.2

	h.	m.	s.
The mean value of T′ =	9	56	13.8
The mean value of T =	9	55	47.2
Difference =			26.6

By the formula,

$$\theta - \theta' = [.0031681^s][\text{no. units in log. k}] \cot \theta'$$

log. .0031681 = 7.50081

log. 668 = log. no. units in log. k = 2.82478

log. cot θ' = 1.09856n

log. ($\theta - \theta'$) = 1.42415n

$\theta - \theta'$ = −26.6s

---

7. ON THE RELATION BETWEEN THE ATOMIC VOLUME OF DIFFERENT METALS, AND THEIR PARAMAGNETIC AND DIAMAGNETIC PROPERTIES. By P. H. VANDERWEYDE, of New York.

WHEN we divide the specific gravity of an elementary substance into its atomic weight, we obtain a quotient which has been called the Atomic Volume. On the supposition that the

so called atomic weights represent really the relative weights of the atoms, these quotients would indicate the relative distances of these atoms. It has been observed that these different quotients lay close together, so that a group of some substances will give a quotient of which the mean is about $3\frac{3}{4}$, others $4\frac{1}{2}$, others 10, and others 20, etc.; but as far as I have been able to ascertain it has never been observed that there exists a most remarkable relation between these quotients and the magnetic properties of the different metals, as the following table shows:

NAME OF METALS.	SPEC. WEIGHT.	ATOMIC WEIGHT.	QUOTIENT.	REMARKS.
Cobalt.	8.5	30	3.53	Paramagnetic at bright white heat.
Iron.	7.8	28	3.59	" only below bright red heat.
Chromium.	7.	26.2	3.74	" " " dark red heat.
Nickel.	8.8	29	3.3	" " " 600° F.
Manganese.	7.1	27	3.80	" " " 4° F.
Palladium.	11.3	53	4.49	Paramagnetic with strong electro-magnets.
Platinum.	21.5	99	4.57	
Osmium.	21.4	100	4.68	
Alumium.	2.56	13.6	5.35	
Cadmium.	8.7	56	6.4	
Magnesium.	1.74	12	7	
*Copper.	8.96	63.4	7.07	Paramagnetic with strong electro-magnets.
Lead.	11.445	103.5	9.12	
Gold.	19.3	196	10.005	
Silver.	10.5	108	10.3	
Calcium.	1.58	20	12.6	
*Mercury.	13.59	200	14.36	Strongly diamagnetic.
Tin.	7.29	118	16.17	
Antimony.	6.7	120.3	17.9	Very strongly diamagnetic.
Bismuth.	9.9	208	21.22	Strongest diamagnetic metal after Faraday.
Sodium.	0.97	23	23.7	
Potassium.	0.86	39	45.35	Strongly diamagnetic.

OBSERVATIONS.

1. The five so called magnetic metals give us the smallest quotients laying all between 3 and 4.

2. The commonly so called non-magnetic metals have all quotients above 4.

3. Faraday proved, with powerful electro-magnets, that next to the five so called magnetic metals, Palladium, Platinum and Osmium possess the strongest paramagnetic properties. They have in the above list, also, the next smallest quotient connected with them which lay between 4 and 5.

*I have doubled the atoms of Copper and Mercury, only to reduce the formula of their lowest oxides to that of the other metals MO, in place of M_2O. For the same reason we must, perhaps, double the atoms of Lead also, which would double its quotient.

4. In the same way that diamagnetism is the opposite of paramagnetism, the larger quotients in the preceding table belong to diamagnetic bodies; for instance, Mercury, Antimony and Bismuth. The last is the strongest diamagnetic substance experimented upon by Faraday, and possesses the largest quotient of any of the heavy metals.

5. If this law holds also for the light metals, Sodium and Potassium must be still stronger diamagnetics, which I found in so far verified by experiment, that bars of these metals enclosed in glass tubes were found to be strongly diamagnetic. However, they have not yet been compared with Bismuth.

6. Of the five magnetic metals, the quotients are the smallest for those which are the most permanently magnetic, even at a high temperature and *vice versa*, with the exception of Nickel.

7. As cooling increases, by contraction, the number expressing the specific gravity, it will consequently decrease this quotient, indicating a decrease in the distance of the atoms, in perfect accordance with the fact that cooling increases the paramagnetic properties.

8. If we were able to cool the other metals so as to increase their specific gravity to such a degree as to have a decided effect on the amount of this quotient, we might perhaps succeed in discovering in more of them paramagnetic qualities, by means of the apparatus used by Faraday.

9. As inversely heating decreases, by expansion, the specific gravity, it will increase this quotient, in accordance to the fact that heat diminishes paramagnetism, and finally destroys it in all metals with the single exception of Cobalt, which has a small quotient and, consequently, can stand some increase.

10. Heating decreasing the paramagnetic qualities with the specific weight and the quotient, may do this to such a degree as to make the body first neutral and then paramagnetic. An instance is known where this takes place, namely, oxygen gas: when cool it is paramagnetic like iron, and when hot diamagnetic bismuth.

11. That this relative distance of atoms (upon which, of course, their specific gravity depends,) is closely related to their magnetism, is again proved by the crystals, which, being always less dense in the direction of their optical axis, and

expanding by heat more in one direction than in another, have been proved by Pluecker to be diamagnetic in the direction of their optical axis or longest axis of crystallization. In some of these crystals this action is so strong that they are influenced by the magnetism of the earth; as for instance, a properly cut crystal of kyanite, which will behave like a compass needle when suspended on an axis, a fact also mentioned by Professor Silliman in the last edition of his excellent textbook on Physics.

Attraction, which pervades all nature, may be classified in four grades, the chemical affinity and the cohesion, acting at distances infinitely or unmeasurably small, the very opposite of gravitation, acting at distances infinitely or unmeasurably large. We may also distinguish between chemical affinity and wherein the last may be overcome by mechanical force, which does not affect chemical combination. Then we have finally the magnetic attraction, which we may consider as the cohesive force, otherwise confined within the surfaces limiting the body, manifesting itself outward at measurable distances. I will not occupy the time of the Association in developing this idea, which finds a strong support in the fact that, by proper electric currents, two pieces of iron, for instance, may be made to adhere with a power as if they were one; and I will only remark that the magnetic attraction appears to be the connecting link between the force of cohesion, acting at infinitely small distances, and that of gravitation, acting at infinitely large distances.

We know that heat will counteract either of these attractions, chemical affinity as well as cohesion, as it has been proved that, at a sufficient high temperature, all bodies are not only gaseous, but that also all compounds are separated, and their constituents reduced to their elementary condition; that magnetic attractions are destroyed, and that even gravitation is counteracted, as heat will enlarge enormously the bulk of a celestial body, diffusing its constituent parts otherwise kept together by gravitation. This last idea, or rather fact, gave origin to Laplace's ingenious hypothesis, that the whole universe was once in a nebular heated condition which by cooling contracted. The modern discoveries in regard to the conser-

vation of forces, have modified this hypothesis and at the same time elevated it into a theory, and we hold now that in place of the nebular matter being hot, and concentrated by cooling, it was cold and concentrated by gravitation, coalescing in different centres of attraction, the motion thus produced being at its partial disappearance in these centres, converted into heat. Afterwards this heat radiated out into space, in the form of the sunbeam and the light of the stars, and as it has been proved that the heat and light of our sun is the source of all life and motion on our planet (with the single exception of the tidal wave), which heat of the sunbeam again owes its existence to the primary gravitation which once caused it to attain its high temperature, we may safely state that all life and motion on this planet is nothing but gravitation in disguise.

Gravitation thus, of which the velocity of transmission surpasses that of light, being literally infinite (astronomy proving that its attraction acts instantaneously at any distance), becomes a fundamental law developing itself in a series of consequences of the utmost diversity.

I do not wish to go into speculations concerning the conclusions which may be drawn from the data I bring before you; speculations about the ultimate composition of matter, nature of atoms, the manifestations of their magnetic attractions or repulsions, etc. To an inquiring mind possessing the knowledge of the latest investigations in cognate branches of research, manifold are the speculations to which it gives rise, and I will be happy if others more able than myself, will also give some attention to the subject I have the honor of bringing before this Association.

---

8. The Steam Boiler. By J. A. Miller, of New York.

INTRODUCTORY REMARKS.

To give a general idea of the principles I wish to advance, I will represent on the blackboard a flask, partly filled with water, over a gas burner, the mouth being secured with a stopper. As soon as the pressure of steam in the interior is suffi-

cient to force out the stopper, not only is all the steam allowed to escape, but the greater portion of the water is forced out. Now this is a regular boiler explosion,—not very disastrous; yet I have made the same experiment with a similar flask, strong enough to stand a pressure of fifty pounds to the square inch, on which I placed a safety-valve, so arranged that it would lift suddenly at thirty pounds pressure. After placing the same over a fire, an assistant and myself watched the operation through a one-inch hole bored through a board placed before us for safety. A few seconds after the flask had been fifteen minutes on the fire, the safety-valve opened entirely, a powerful jet of steam was forced out of the opening, and instantly after the flask bursted with a loud report, shattering every pane of glass in the windows, and destroying the plastering of the ceiling. The time between the two explosions was infinitely short, and seemed to me just sufficient to force the water against the upper part of the flask, when the whole was shattered into fragments.

If we place into the first flask a cone, and divide the water into a thin film near the heating surface and a larger body of water not exposed to heat; and then place a little powdered amber in the water, we find, on placing the flask on the burner, that the cone of glass is heated first, and thereby heats the water in contact with the same. But a large portion of the heat passes through, or rather is radicated from the inner surface of the flask, which rays being intercepted by the shield, heat the same, and we have a thin film of water in contact with two heating surfaces. This water soon becomes warm, and, of a consequence, lighter; it is, therefore, forced upward by the cooler and heavier water in the larger space, hereby producing circulation of the water at first, as will be observed by the particles of amber slowly, yet rapidly, increasing as the heat increases. You will also observe that as the water in the larger space moves but slowly, the amber is gradually deposited in the pocket formed by the shield, and no deposit can collect near or on the heating surface, thereby always, and with any kind of water, keeping the heating surface clean and efficient. Soon the pressure will be sufficient to force out the stopper. Now, what is the result? The stopper is out,

and all the water above the shield is hanging in drops on the ceiling. Why not the water below the shield, nor even below the top line of the same? Simply because it contains no steam, but mere solid water; whereas, in the first experiment, there was as much steam, bulk for bulk, in the water as in the steam space.

Now, the third flask is essentially like the first and second, only the shield is carried up to or a little above the water line. When we place it on the gas burner the whole heat is concentrated on the thin film of water between the flask and the shield. The steam is at once carried into the steam space, not by any exertion or force of its own, but by the superior weight of the cooler water. We first get the steam at first cost. Second, we have a rapid absorption of the heat. Third, a perfect guarantee that water is always in contact with the heating surface. Fourth, we have a circulation of water exactly proportioned to the amount of heat given up to the surface, thereby ensuring always, and under all circumstances, a uniform temperature in the boiler plate, no matter what the heat of the fire, and can have no burning out of the same. We have no deposit, but on the contrary, under all circumstances, a bright surface exposed to the fire. And last, we have a perfect, simple and natural reservoir for the collection of any mineral, vegetable or saline deposits, or impurities, contained in the water. Observe well when now the stopper is forced out. I shall hold my hands over the opening, for as no water can be forced out, the steam being on top there is nothing to scald me,—an experiment I should not like to perform in the first instance. The stopper is out, the steam has not scalded my hands, and, as you will easily perceive, the water line is not the least altered.

This, Mr. President and Gentlemen, is the principle I claim, and which I incorporate into all improvements in steam boilers.

The true end, aim, and object of science is the careful investigation of all things that to the mass of mankind are mysterious and unaccountable. Science is never satisfied to merely know a thing *is*, but is ever searching for the cause. Millions of men had seen apples fall before Newton proved to the

world the cause; nor is the scientific mind ever satisfied with the simple knowledge of effect produced, but labors unceasingly to discover the laws or causes which produce these effects.

A family in Germany dies after eating ham. They were in ordinary health, and their death could not be explained by ordinary known causes. The facts in the case are hardly stated, when hundreds of scientific men, setting aside all other occupations, concentrate their minds on the investigation of the true cause which produced this effect; and the result is the discovery of a Microscopic Parasite as the cause of death, and also that a temperature of 180° removes this cause, and renders the food harmless.

It is this thirst for positive knowledge that leads men to brave the frosts of the Arctic Ocean, and the burning sands of Africa: it is this that caused Faraday to leave his laboratory and cross two oceans to investigate the Geysers of Iceland; and in this investigation, not only did he discover the laws which govern these periodical discharges of large masses of water, but he showed us one of the main causes of steam boiler explosions.

The steam boiler, as the source of power for the steam engine, has become as necessary to man as the elements. Without it this convention of men devoted to science, from all sections of this vast continent, could not have been held. None could have spared the time, nor endured the fatigue of the journey; nay, even this "Empire City of the West," which has so hospitably received us, and opened to our enjoyment her magnificent hotels, public and private buildings, would not, could not, have existed without its aid.

Invaluable as this power is to man, it would be more so if positively, absolutely safe, which is not the case. Yet who doubts that it can be made so, as safe as a water-wheel?

We have seen how soon science discovered the cause of the death of the German family, and instantly showed a safe and simple remedy. Let the scientific men of our time as carefully and persistently investigate the steam boiler, and the true cause of danger once discovered, the remedy will be at hand. But to arrive at the true cause *nothing* must be left to meritorious

conjecture. All actions of heat and water must be carefully investigated, and remedies based on *authenticated facts*.

The ablest document ever presented to the scientific world on this subject, was the Report of a Committee of the Franklin Institute of Philadelphia, by the honored and lamented member of this Association, the late Alexander Dallas Bache. This report has done much to prevent the reckless use of steam boilers, and to it we owe the passage by Congress of the laws for the inspection of steam boilers. Yet, even these laws, stringent as they are, have not reached the whole case. Daily, widows and orphans lament the loss of some dear one sacrificed to this insatiable Moloch. The field is therefore still open, and strict, careful, and patient investigation still a *duty*.

For this purpose I would lay before you the result of investigations carried on during several years, and under the most favorable circumstances, having over 200 steam boilers under personal supervision, boilers constructed under every principle and of every form.

The reliable causes of explosions may be summed up as follows: —

1st. Gradually increasing pressure.

2d. Sudden increase of pressure by exposed highly heated surfaces.

3d. Incrustation.

4th. Sudden contraction and expansion of metal.

The more mysterious causes assigned, such as decomposition, creation of explosive gases, electricity, &c., we will not here consider, since they are purely *speculative*.

Referring to the above well known and *reliable* causes, the first, "Gradually increasing pressure," seems at first sight to be easily disposed of by first testing the strength of the boiler, and then providing a properly proportioned safety-valve. Yet such is not really the case, and a close inspection will distinctly show the reason.

Before us we have a number of drawings of boilers shown in section.*

No. 1, shows a boiler filled with water heated to nearly 212°. When we first apply heat no perceptible change takes

* These diagrams were exhibited at the meeting.

[illegible]

Now whenever the pressure equilibrium cannot take place until the pressure of steam in the water is in excess of the pressure of the steam in the steam space, for the steam in the water must have its way through a medium 2,000 times more dense than the steam itself, and consequently, if the pressure of the steam in the steam space but a trifle less, for the steam formed near the boiler can rise easily. When it reaches the water surface it occupies a space some 2,000 times greater than it did when it was water. It is self evident, therefore, that a much larger quantity of steam is actually contained in the water itself than in the steam space. Remembering this fact, let us now open the safety-valve suddenly, and what is the result? a sudden relief of pressure, a violent rising of water under the safety-valve, and a sudden supply of steam from the body of the water. If, now, we have exposed fires, or highly heated metal, with which the steam can come in contact, we will have such a development of force as no steam boiler can withstand, and a sudden rupture of the weakest part of the boiler is the unavoidable result. Now a rupture of any part of the boiler ought to have the same effect as the action of the safety-valve; for the common cry is, "that safety-valves are not of sufficient area to allow the free escape of steam; whereas, a rupture is generally amply large, and yet in truth, the effects are precisely the same. When a boiler bursts, all the water is converted into steam, not purely *dry*

steam, but steam mixed with minutely divided particles of water.

The immense force exerted by the explosion of a boiler is looked upon by many as so stupendous that they cannot conceive of the possibility that simple steam can produce such results; and even theories which assume formation of explosive gases, ignited by some mysterious cause, are favorably received.

Anxious to obtain some reliable data regarding the actual power stored in a steam boiler, I made the following experiment.

Selecting four boilers, favorably located and well set, arranged with proper dampers, and well covered so as to retain the heat, I had the steam increased to one hundred pounds per square inch above the atmospheric pressure. Steam was then admitted to a Corliss Steam Engine, which operated six machines, which as it was carefully ascertained, consumed 150 horse-power. After running the engine for one hour the conditions were as follows:

Pressure of steam one hundred pounds, and three gauges of water. The dampers were then carefully closed, the fires drawn, and the engine started. At first a high grade of expansion was used, gradually decreasing until in forty nine minutes the steam followed nearly the whole stroke, or in other words, nearly an entire cylinder full of steam was used during each stroke of the piston. We then stopped two of the machines, working twenty seven minutes; throwing off two more machines we worked them nineteen minutes, when the engine stopped, with a steam pressure in the boiler of twenty three pounds. Water line, between the second and lower gauge. This gives us a power of 150 horses for forty nine minutes, one hundred for twenty seven minutes, and fifty horse-power for nineteen minutes.

When we now consider, first, that in case of an explosion, the whole of this force would have been exerted in an instant, or say one second, we find that the power exerted would have been equal to 660 000 horses, and when we remember the power lost in the engine and that still remaining in the boiler, 1 000-000 horse-power would not be overrating the actual result. So

stupendous is this power that the human mind can scarcely conceive its immensity. Now, when such is the power contained in a steam boiler, what need have we for looking for any solution in the mysterious working of the electric fluid, or production of explosive gases? The power stored in the water is much more than sufficient to produce the worst and most disastrous effects.

To illustrate the action of the steam and water when the safety-valve is suddenly opened, I have drawn a line in the section of the boiler in which a simple perforated pipe is shown, which pipe should be used on all boilers; first, as a "dry pipe," and also to distribute the relief of pressure over the whole surface of the water level, for it is well known that when the water has been at rest, the starting of the engine, or the opening of the safety-valve, is the time at which most explosions take place.

Incrustration is the worst enemy of the steam boiler, for in all cases it is a poor conductor of heat, and by preventing the contact of the water with the boiler plates the latter become overheated and consequently rapidly destroyed; particularly as the concave form of our boilers causes the collection of sediment in the lowest point, and as this is the portion of the boiler that is directly over the fire the iron is soon destroyed: entire plates become overheated; the expansion causes the scale to break, when water comes in contact with highly heated, burned and weakened metal; sudden formation of steam is caused, sudden increase of pressure follows, causing rupture, and with it all the disastrous consequences of explosion.

Having thus pointed out the true causes of boiler explosions, the next step is to point out the remedy, and to do this let us return to the water in the boiler.

When we take a vessel containing milk, syrup or any other fluid and place the same over the fire, we find that soon after boiling commences the liquid foams and is in danger of "boiling over," as it is called. If we then stir the liquid large volumes of steam are set free, and boiling instantly ceases. Now what we here do is simply by mechanical means assist the steam contained in the liquid to escape. If we take a

vessel in which liquid is violently boiling and pour the contents slowly into another vessel, we produce the same effect by allowing the steam to escape. If we place diaphragms in a boiler we produce a similar effect by creating currents circulating upwards over the edge of the diaphragm, caused by the difference in the specific gravity of "Water filled with steam" and water without steam. Then, if we go still farther, and provide an "eddy" where the water can be comparatively quiet, we shall also provide for the collection of sediment in pocket surrounded by water and not in contact with the fire, as shown in this drawing.

The results are, first, that all steam as soon as generated enters at once into the steam space, and that the sediment being located in a pocket away from the heating surface, the water having at all times free access to the same, making *any* overheating of the plates an *impossibility*. We have in fact, steam, water and sediment, all in their proper positions; we have a bright and efficient heating surface, rapid absorption of the corresponding equal distribution of temperature, and a safe and efficient steam boiler.

Circulating of water in steam boilers has been frequently attempted, and one of these attempts, from which immense results were anticipated, is illustrated in figure 1, of this diagram. This is known as the "Perkins Boiler." It was the cause of an able paper by the late Professor Bache, in which he most conclusively shows, that no matter how desirable the circulation of water in steam boilers is, the Perkins boiler, designed especially to this end, may lay no claim whatever to its benefits.

Circulation of water depends upon the difference in weight of two columns, the lighter being displaced by the heavier, so that to produce the same, we must have one column exposed to heat to make it lighter, whereas the other column must be separated or protected from heat, so that in nowise will its descent be prevented. A glance at the drawing shows that in the Perkins boiler no such effect could be produced. First, if we consider the central column as ascending, being exposed to the most intense heat of the fire, we also recognize the fact that the annular space cannot permit the free descent of the

water as that also is exposed to heat, and in both the lateral and annular spaces steam is constantly formed, and the introduction of the diaphragm retards rather than accelerates the production of steam. Professor Bache says, "that the introduction of the cylinder or diaphragm caused violent foaming, so much so, that he was compelled to affix a funnel-shaped cylinder to the top of the vessel to prevent the water from being forced over its sides."

Now this violent foaming is the direct result of the division of the water, by which steam formed at the bottom, rising rapidly and expanding, carries solid water with it. That under such circumstances a certain quantity of fuel cannot evaporate as much water, is evident from the fact that heat being power, all power lost in forcing the steam through the water is so much heat wasted.

This other boiler, in which the desired conditions are proved, shows plainly where the Perkins boiler *failed*, and how a perfect circulation of the water may be obtained, causing a rapid upward and a slow downward current, and provision also made for the collection of impurities below the line of fire. That the two systems differ in the most essential points, is apparent, as well as that one fully accomplishes what the other merely attempted.

In conclusion I would lay down a positive law for the construction of steam boilers, which under all circumstances *must* be absolutely free from disastrous explosions.

1st. A steam boiler should be constructed of the largest practical number of units, so that the giving way of one of them under excessive pressure, relieves the whole.

2d. Each unit must constitute in itself a complete steam boiler, having its water space exposed to the fire, its steam space above the fire, and a receptacle for the collection of sediment below the influence of the fire.

3d. All heating surface must be vertical, or nearly so, at or nearly at right angles to the line of fire.

4th. Free and perfect circulation of the water must be secured by having thin films of water exposed to the heat, ascending, and large waterspaces not exposed to the heat descending, thereby also securing perfect separation of steam from the water.

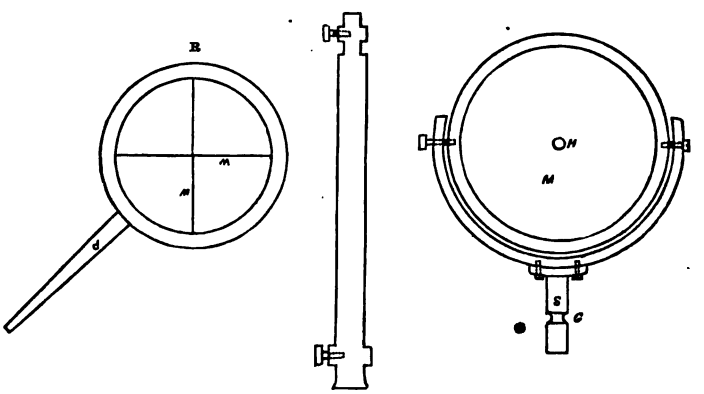
Steam boilers constructed on these principles will, in the hands of even the most inexperienced persons, be free from danger, and at the same time insure the highest economy in fuel.

The daily recurring loss of life, the destruction of property, the insecurity of travel, together with the daily increasing number of steam boilers, must be my apology for occupying so much valuable time. If, by my labor of years in investigating this subject, I may be instrumental in saving but one human life, my labor will not have been in vain.

---

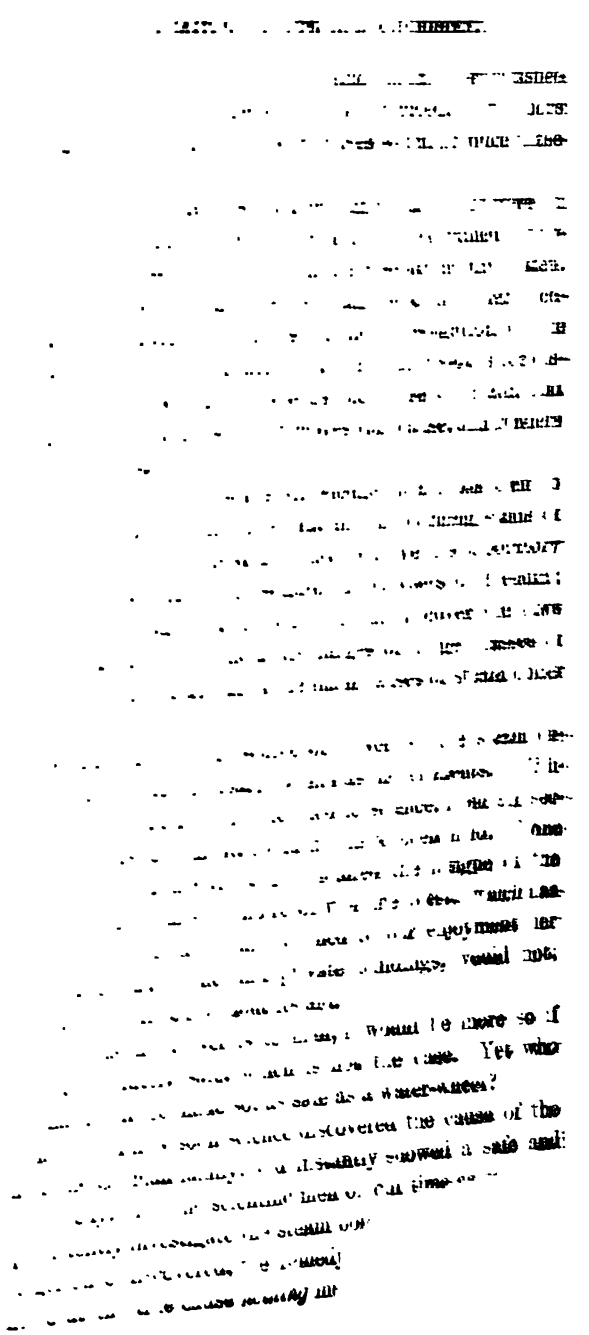
9. On the Heliostat, and Method of running true or Meridian Lines. By Michael McDermott, Civil Engineer and City Surveyor of Chicago.

This instrument consists of a mirror, pole, Jacob's-staff or steel rod, and a brass ring with cross wires. The annexed figures represent one used by me for some time, which has been made for me by Mr. Kratzenstein of Chicago, and cost but nine dollars.



The mirror (M) is of the best French mirror material, $3\frac{3}{4}$ inches in diameter, set in a brass bronzed frame or ring, $4\frac{1}{2}''$ outer, and $3\frac{3}{4}''$ inner diameter, and $\frac{3}{10}$ of an inch thick. This mirror is set into a semi-circular ring $\frac{4}{10}''$ thick, leaving a space

cient to f
to escap
Now this
yet I ha
strong e
inch, on
would li
the same
ation th
before u
fifteen
a power
instantl
ing eve
plasteri
sions w
force th
whole
If w
into a
of wat
dered
the bu
heats t
of the
inner
shield,
contac
warm
upwa
hereby
obser
creasi
the w
gradu
depos
alway
clean
out th



conjecture. All actions of heat and water must be carefully investigated, and remedies based on *authenticated facts*.

The ablest document ever presented to the scientific world on this subject, was the Report of a Committee of the Franklin Institute of Philadelphia, by the honored and lamented member of this Association, the late Alexander Dallas Bache. This report has done much to prevent the reckless use of steam boilers, and to it we owe the passage by Congress of the laws for the inspection of steam boilers. Yet, even these laws, stringent as they are, have not reached the whole case. Daily, widows and orphans lament the loss of some dear one sacrificed to this insatiable Moloch. The field is therefore still open, and strict, careful, and patient investigation still a *duty*.

For this purpose I would lay before you the result of investigations carried on during several years, and under the most favorable circumstances, having over 200 steam boilers under personal supervision, boilers constructed under every principle and of every form.

The reliable causes of explosions may be summed up as follows: —

1st. Gradually increasing pressure.

2d. Sudden increase of pressure by exposed highly heated surfaces.

3d. Incrustation.

4th. Sudden contraction and expansion of metal.

The more mysterious causes assigned, such as decomposition, creation of explosive gases, electricity, &c., we will not here consider, since they are purely *speculative*.

Referring to the al[illegible]ll known and *reliable* causes, the first, "Gradually in[illegible]ressure," seems at first sight to be easily disposed [illegible] testing th[illegible]of the boiler, and then provi[illegible]-valve. Yet such is not re[illegible]tinctly show the [illegible]

Bef[illegible]own in sect[illegible]

[illegible] water heated [illegible] nearly [illegible] no percepti[illegible] takes [illegible]

place, except that the temperature is gradually increased, or in other words, the water is gradually heated until the entire mass is at 212° under ordinary atmospheric pressure. When this condition is reached, we may do either one of two opposite things, and produce the same result, viz.: we may add more heat to the water and produce ebullition, or we may relieve the atmospheric pressure by means of the air pump, which produces the same result. Both will expel a portion of the steam contained in the water. Now if a fire is maintained under the boiler, the steam generated accumulates in the steam space and gradually increases the pressure on the water, which pressure in its turn prevents ebullition, until more heat is added; as, therefore, the pressure increases so will the temperature increase likewise.

Now whatever the pressure, ebullition cannot take place until the pressure of steam in the water is in excess of the pressure of the steam in the steam space, for the steam in the water must force its way through a medium 813 times more dense than the steam itself, and subjected to the pressure of the steam in the steam space, nor is even this all; for the steam formed near the boiler iron must expand. When it reaches the water surface it occupies a space some 1700 times greater than it did when it was water. It is self evident, therefore, that a much larger quantity of steam is actually contained in the water itself, than in the steam space. Remembering this fact, let us now open the safety-valve suddenly, and what is the result? a sudden relief of pressure, a violent rising of water under the safety-valve, and a sudden supply of steam from the body of the water. If, now, we have exposed flues, or highly heated metal, with which the steam can come in contact, we will have such a development of force as no steam boiler can withstand, and a sudden rupture of the weakest part of the boiler is the unavoidable result. Now a rupture of any part of the boiler ought to have the same effect as the action of the safety-valve; for the common cry is, "that safety-valves are not of sufficient area to allow the free escape of steam; whereas, a rupture is generally amply large, and yet in truth, the effects are precisely the same. When a boiler bursts, all the water is converted into steam, not purely *dry*

steam, but steam mixed with minutely divided particles of water.

The immense force exerted by the explosion of a boiler is looked upon by many as so stupendous that they cannot conceive of the possibility that simple steam can produce such results; and even theories which assume formation of explosive gases, ignited by some mysterious cause, are favorably received.

Anxious to obtain some reliable data regarding the actual power stored in a steam boiler, I made the following experiment.

Selecting four boilers, favorably located and well set, arranged with proper dampers, and well covered so as to retain the heat, I had the steam increased to one hundred pounds per square inch above the atmospheric pressure. Steam was then admitted to a Corliss Steam Engine, which operated six machines, which as it was carefully ascertained, consumed 150 horse-power. After running the engine for one hour the conditions were as follows:

Pressure of steam one hundred pounds, and three gauges of water. The dampers were then carefully closed, the fires drawn, and the engine started. At first a high grade of expansion was used, gradually decreasing until in forty nine minutes the steam followed nearly the whole stroke, or in other words, nearly an entire cylinder full of steam was used during each stroke of the piston. We then stopped two of the machines, working twenty seven minutes; throwing off two more machines we worked them nineteen minutes, when the engine stopped, with a steam pressure in the boiler of twenty three pounds. Water line, between the second and lower gauge. This gives us a power of 150 horses for forty nine minutes, one hundred for twenty seven minutes, and fifty horse-power for nineteen minutes.

When we now consider, first, that in case of an explosion, the whole of this force would have been exerted in an instant, or say one second, we find that the power exerted would have been equal to 660 000 horses, and when we remember the power lost in the engine and that still remaining in the boiler, 1 000-000 horse-power would not be overrating the actual result. So

stupendous is this power that the human mind can scarcely conceive its immensity. Now, when such is the power contained in a steam boiler, what need have we for looking for any solution in the mysterious working of the electric fluid, or production of explosive gases? The power stored in the water is much more than sufficient to produce the worst and most disastrous effects.

To illustrate the action of the steam and water when the safety-valve is suddenly opened, I have drawn a line in the section of the boiler in which a simple perforated pipe is shown, which pipe should be used on all boilers; first, as a "dry pipe," and also to distribute the relief of pressure over the whole surface of the water level, for it is well known that when the water has been at rest, the starting of the engine, or the opening of the safety-valve, is the time at which most explosions take place.

Incrustration is the worst enemy of the steam boiler, for in all cases it is a poor conductor of heat, and by preventing the contact of the water with the boiler plates the latter become overheated and consequently rapidly destroyed; particularly as the concave form of our boilers causes the collection of sediment in the lowest point, and as this is the portion of the boiler that is directly over the fire the iron is soon destroyed: entire plates become overheated; the expansion causes the scale to break, when water comes in contact with highly heated, burned and weakened metal; sudden formation of steam is caused, sudden increase of pressure follows, causing rupture, and with it all the disastrous consequences of explosion.

Having thus pointed out the true causes of boiler explosions, the next step is to point out the remedy, and to do this let us return to the water in the boiler.

When we take a vessel containing milk, syrup or any other fluid and place the same over the fire, we find that soon after boiling commences the liquid foams and is in danger of "boiling over," as it is called. If we then stir the liquid large volumes of steam are set free, and boiling instantly ceases. Now what we here do is simply by mechanical means assist the steam contained in the liquid to escape. If we take a

vessel in which liquid is violently boiling and pour the contents slowly into another vessel, we produce the same effect by allowing the steam to escape. If we place diaphragms in a boiler we produce a similar effect by creating currents circulating upwards over the edge of the diaphragm, caused by the difference in the specific gravity of "Water filled with steam" and water without steam. Then, if we go still farther, and provide an "eddy" where the water can be comparatively quiet, we shall also provide for the collection of sediment in pocket surrounded by water and not in contact with the fire, as shown in this drawing.

The results are, first, that all steam as soon as generated enters at once into the steam space, and that the sediment being located in a pocket away from the heating surface, the water having at all times free access to the same, making *any* overheating of the plates an *impossibility*. We have in fact, steam, water and sediment, all in their proper positions; we have a bright and efficient heating surface, rapid absorption of the corresponding equal distribution of temperature, and a safe and efficient steam boiler.

Circulating of water in steam boilers has been frequently attempted, and one of these attempts, from which immense results were anticipated, is illustrated in figure 1, of this diagram. This is known as the "Perkins Boiler." It was the cause of an able paper by the late Professor Bache, in which he most conclusively shows, that no matter how desirable the circulation of water in steam boilers is, the Perkins boiler, designed especially to this end, may lay no claim whatever to its benefits.

Circulation of water depends upon the difference in weight of two columns, the lighter being displaced by the heavier, so that to produce the same, we must have one column exposed to heat to make it lighter, whereas the other column must be separated or protected from heat, so that in nowise will its descent be prevented. A glance at the drawing shows that in the Perkins boiler no such effect could be produced. First, if we consider the central column as ascending, being exposed to the most intense heat of the fire, we also recognize the fact that the annular space cannot permit the free descent of the

water as that also is exposed to heat, and in both the lateral and annular spaces steam is constantly formed, and the introduction of the diaphragm retards rather than accelerates the production of steam. Professor Bache says, "that the introduction of the cylinder or diaphragm caused violent foaming, so much so, that he was compelled to affix a funnel-shaped cylinder to the top of the vessel to prevent the water from being forced over its sides."

Now this violent foaming is the direct result of the division of the water, by which steam formed at the bottom, rising rapidly and expanding, carries solid water with it. That under such circumstances a certain quantity of fuel cannot evaporate as much water, is evident from the fact that heat being power, all power lost in forcing the steam through the water is so much heat wasted.

This other boiler, in which the desired conditions are proved, shows plainly where the Perkins boiler *failed*, and how a perfect circulation of the water may be obtained, causing a rapid upward and a slow downward current, and provision also made for the collection of impurities below the line of fire. That the two systems differ in the most essential points, is apparent, as well as that one fully accomplishes what the other merely attempted.

In conclusion I would lay down a positive law for the construction of steam boilers, which under all circumstances *must* be absolutely free from disastrous explosions.

1st. A steam boiler should be constructed of the largest practical number of units, so that the giving way of one of them under excessive pressure, relieves the whole.

2d. Each unit must constitute in itself a complete steam boiler, having its water space exposed to the fire, its steam space above the fire, and a receptacle for the collection of sediment below the influence of the fire.

3d. All heating surface must be vertical, or nearly so, at or nearly at right angles to the line of fire.

4th. Free and perfect circulation of the water must be secured by having thin films of water exposed to the heat, ascending, and large waterspaces not exposed to the heat descending, thereby also securing perfect separation of steam from the water.

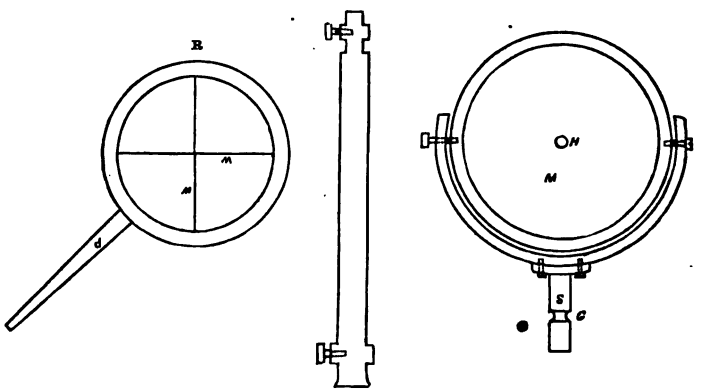
Steam boilers constructed on these principles will, in the hands of even the most inexperienced persons, be free from danger, and at the same time insure the highest economy in fuel.

The daily recurring loss of life, the destruction of property, the insecurity of travel, together with the daily increasing number of steam boilers, must be my apology for occupying so much valuable time. If, by my labor of years in investigating this subject, I may be instrumental in saving but one human life, my labor will not have been in vain.

---

9. On the Heliostat, and Method of running true or Meridian Lines. By Michael McDermott, Civil Engineer and City Surveyor of Chicago.

This instrument consists of a mirror, pole, Jacob's-staff or steel rod, and a brass ring with cross wires. The annexed figures represent one used by me for some time, which has been made for me by Mr. Kratzenstein of Chicago, and cost but nine dollars.



The mirror (M) is of the best French mirror material, $3\frac{3}{4}$ inches in diameter, set in a brass bronzed frame or ring, $4\frac{1}{2}''$ outer, and $3\frac{3}{4}''$ inner diameter, and $\frac{3}{10}$ of an inch thick. This mirror is set into a semi-circular ring $\frac{4}{10}''$ thick, leaving a space

between it and the ring of $\frac{2}{10}$ of an inch. Both are connected by two screws, one of which is a clamping screw. The semi-circular, or outer part, is attached to a circular piece of the same dimensions as the outer piece, $1\frac{1}{2}''$ long, and being part of an axis $9\frac{1}{2}''$ long, into which there is a groove to receive the clamping screw from the tube or socket. This piece is marked S on figure.

The socket or tube is 8″ long, $\frac{1}{2}''$ inner diameter, having two clamping screws, one to clamp the whole apparatus to the Jacob's-staff, or rod, and the other to allow the mirror to be turned in any direction. By these three clamping screws the instrument can be raised and turned in any direction. The back of the mirror is lined with brass, in the centre of which there is a small hole, opposite to which the foil is removed from the glass. A pole is set near the station to be observed so that the centre of a brass ring (R) will be over it. This ring is to be a little less than the diameter of the mirror, and so made as to have a piece of steel wire, 2″ long, attached to it, by which it can be fastened to the side of a pole which has several holes at four or six inches apart. The mirror is now set on the Jacob's-staff about forty feet behind the pole at station A, and making the heliostat and pole approximately in direction of station B, where the observer is ready to take an angle to A, and also provided with another heliostat. The ring is adjusted to the pole, and the centre of the mirror and that of the ring set as near as may be in direction of B. The assistant at station A makes the concentrated rays of the sun pass through the ring about ten minutes of time. If he does not receive a return flash from B, he changes the ring higher or lower and gives other flashes, and so continue till B returns a flash, then from B's flash A is able to adjust his heliostat more accurately. By a preconcerted code of signals B orders A to proceed to the next given trigonometrical station, and so continue.

The heliostat and pouch now exhibited weigh but $3\frac{3}{4}$ pounds.

A mirror of four inches will be seen forty miles, and one of eight to ten inches one hundred miles, where the stations will be sufficiently elevated.

MERIDIAN LINES.

All surveyors know that to reëstablish a lost line by means of the magnetic needle is impossible, as the annual and daily variations, combined with local causes, are obstacles too difficult to overcome. In Canada, magnetic surveys are not legal. There the bearings of all lines are determined astronomically, and consequently can be easily established at any future time with the *utmost accuracy*.

In this country the lines are supposed (since 1805), in the Western States to run North and South, and the lines running East and West to close within a limited number of links on the North and South lines. However, owing to *various causes*, the lines are found twisted out of position. Here, in order to locate a line, we must have its extreme points or corners, whereas in Canada one point is sufficient.

The present method of making surveys is very wrong and expensive, and is the cause of thousands of law suits respecting boundary lines. Let the surveyors only know that the Supreme Courts of many of our States have decided that where the lines in a deed are given South, North, East or West, such lines must be run due North or South. This is plainly laid down in Section 2 of the act of 11th Feb.,,1805. (See United States Statutes at large, Vol. II, p. 313.) Hence appears the necessity of taking the true bearing of every line astronomically, or by referring it to some near line whose true bearing has been found.

In the Northern Hemisphere the stars in Ursa-minoris give us an opportunity of taking the meridian several times during the night.

It has been generally supposed by surveyors, that when Polaris and Alioth in Ursa-majoris are on the same vertical plane or line, that then Polaris is on the meridian; but such is not the case.

In Chicago, latitude 41° 50′ 30″, on the first of January, 1867, the bearing of Polaris, when vertical with Alioth, was North 0° 10′ 21″ East. (See McDermott's Civil Engineer and Surveyors' Manual, sec. 385, for method of calculation.) For a table showing the azimuth of Polaris when in the above posi-

tion, in latitudes 2° to 70°, and for the years 1870, 1880, 1890, 1900, 1910, 1920, 1930 and 1940, see his table XXIV.

In the Southern Hemisphere there is no bright star near the South Pole. I have calculated the greatest azimuths of Sigma in Octantis, a star of the sixth magnitude, from latitude 1° to 70°, and for Polar distances 40′–45′ and 50′. Although the great Creator has not given any bright star near the South Pole, He has given Alpha in Crucis, and Beta in Hydri; the former in the foot of the cross, the latter in the tail of the serpent, and so situated that, when they are on the same vertical plane, the bearing of Alpha Crucis is very small, and its annual variation a mere nothing. For example I give part of my table XXV.

LAT.	YEARS.							Azimuths, or bearings all South east of the meridian.
	1850	1900	1950	2000	2050	2100	2150	
12°	1′.12″	1′.15″	1′.19″	1′.43″	2′.15″	2′.58″	3′.53″	
14	1.12	1.15	1.20	1.43	2.16	3.00	3.56	
&c.	&c.	&c.	&c.	&c.	&c.	&c.	&c.	
20	1′.14″	1′.17″	1′.25″	1′.48″	2′.20″	3′.6″	4′.2″	
22	1.15	1.18	1.26	1.50	2.22	3.8	4.4	

From this it appears that in latitude 12° it will require fifty years to make a change of three seconds of a degree in azimuth; also a change of 10° in latitude to make the same small difference. The placing of these two bright stars could not be the work of chance or accident, but that the cross, the emblem of and guide to salvation, should be an infallible guide in Surveying and Navigation, and there placed by the Great Omnipotent Architect, as the most perfect guide now known.

Table XXIV in McDermott's Manual shows the azimuths of Polaris when vertical with Alioth in Ursa-majoris for 1870, and for every ten years after till 1940, and for every 2° of latitude from the equator to latitude 40, and then for every degree from 40° to 70°.

EXAMPLE IN BEARINGS ALL NORTH-EAST.

LAT.	DATE.		Nearly a change of one minute of a degree in ten years, and about ten seconds in a change of 1° of latitude.
	1870	1880	
40	10′.25″	11′.22″	
41	10.34	11.32	
42	10.44	11.42	
43	10.53	11.53	

Time of certain North circumpolar stars being at their greatest azimuths, or elongations from the meridian, on the first of January, 1867, and of Polaris and Alioth in Ursa-majoris being vertical with one another.

		TIME.
		h. m.
Alpha Ursa-minoris (Polaris),	East of the Meridian,	0.31
Beta " (Kochab),	West " "	1.10
Beta Draconis,	" " "	1.49
Gama "	" " "	2. 8
Alpha Cassiopeæ,	East " "	2.18
Zeta Ursa-minoris	West " "	2.22
Epsilon "	" " "	3.48
Delta "	" " "	5.19
Polaris, vertical	with Alioth,	6. 5
Alpha Cephi,	West of the "	6.38
Gama "	" "	10. 2
Phi Ursa-majoris,	East " "	10.34
Alpha "	" " "	12. 2
Polaris,	West " "	12.21
Beta Ursa-minoris,	East " "	15. 4
Epsilon "	" " "	15.48
Xi "	" " "	16.43
Phi Ursa-majoris,	West " "	17.15
Delta "	East " "	17.43
Beta Draconis,	" "	19.37
Gama "	" " "	20.10
Gama Ursa-majoris,	West " "	20.24
Alpha "	" " "	20.21
Alpha Cephi,	East " "	22.25
Gama "	" " "	23.38

The above is to the nearest minute; but in the table it is given to seconds; also the meridian passage and time to greatest azimuth.

In this method an error in the latitude of half a degree would be about one-half a minute in azimuth of Polaris in latitude 41°. But as the latitude can be taken with little delay, there need be no cause of any sensible error, and lines can be run much closer than now. I calculate the time from the meridian passage to the greatest elongation of the star, and its altitude when at that position. This requires to have the true time, which I find by a new method, as follows:

I calculate the time to or from the meridian passage to the greatest elongation reduced to the sidereal time of station, which reduce to mean time. I next find the true altitude of the star when at its greatest azimuth, to which add the correction for refraction from my table XVII. I take the temperature of the thermometer and approximate that of the barometer, which ranges in clear weather from 29½ to 30. I add this correction for refraction, to find the apparent altitude. I observe when the star is at this altitude and compare it with the

calculated time, and thus correct mine, by which I am able to know the precise time of the required star being at its greatest azimuth, and having the altitude as a check, there can be no method to give greater accuracy.

To find the greatest azimuth = Rad. 10 + Sin Polar Distance, less cos. Lat. = sine of greatest azimuth.

Altitude at greatest azimuth = Sin. Lat. + 10, less cos. Polar Distance = sine of the altitude.

To find the time from meridian passage to the greatest azimuth. Tan. polar dist. + tan. Lat., less 10, = cos. hour angle in space which change into time, gives the sidereal time. (Polaris and Alioth are always on opposite sides of the pole.)

In taking the bearing of a line we have on the object end of our telescope a reflector made thus: — a piece of sheet brass $\frac{1}{2}'$ wide is made to fit the telescope, as A. C is made with an elliptical hole and fastened to A, making an angle of about 45°. There is a space between A and C about $\frac{1}{8}''$. This is put on the telescope. The assistant holds a lamp by which the light can be thrown on the surface C around the hole, and regulated without the least difficulty. We observe the star in the required position, lower the telescope and make a permanent mark in that direction, about ten chains in advance. Then we drive a picket with its top finely pointed, or a stake and nail, to be used on the next day on which we set off the angle required by the instrument, by repeating it two or three times; or we measure the distance from the instrument to the picket and there set off the tangent of the required angle, thus insuring the greatest accuracy. Or let us suppose the angle to be set off = 11° 43′. We set off the 11° and measure ten chains, calculate the tangents of 43′, and set it off with a foot rule. Hence, if we set this small tangent off at twenty chains from the stake, we can run *a distance of six miles to an inch*, by using fine pointed pickets whose tops are set in range. I find whitewashed laths excellent. At the station or picket in advance we use a steel rod five feet long, on the top of which is a disk of sheet iron or pasteboard, behind which our assistant holds his lamp till we see the light at the point or apex of the disk. The open space in disk is one inch at bottom, three inches high, and coming to a point. At the top a tube is attached to fit the rod.

10. ON THE PRINCIPLES OF STATISTICS AS APPLIED TO THE CENSUS. By FRANKLIN B. HOUGH, of Lowville, N. Y., *Superintendent of the New York State Census of* 1855 and 1865.

THE near approach of the period fixed by our Constitution for another National census will justify, at this time, a careful consideration of the methods hitherto employed in making enumerations of this kind, and some of the details of organization that should be considered, in order that the results may be such as may command the public confidence.

I do not propose to undertake an historical review of census systems, nor do I deem it necessary to present arguments to prove that unless the inquiries of the census are carefully and judiciously arranged, and thoroughly and uniformly reported, the deductions therefrom will be comparatively worthless, and even less trusty than mere estimates founded upon general observation.

It may be proper, in passing, to notice the fact, that although returns of population more or less accurate had been previously made through government agencies, the United States was the first country in modern times that provided for a precise and full return of the number of its inhabitants at decennial periods. The necessity of an exact basis upon which representation could be equalized, led to the provision requiring that "within three years after the first meeting of the Congress of the United States, and within every subsequent term of ten years," an actual enumeration of the population should be made in such manner as the law might direct. Under this authority we have had eight returns, commencing with 1790.

In Great Britain, the first regular census was taken in 1801, and in each succeeding year whose unit figure is 1, a full census has been reported. In France, a census was taken in 1801, 1806, 1821, and has been continued at intervals of five years since. The custom has since been adopted in most European countries, in many European colonies, and in several of the States of our Union.*

*Of the thirty-seven states, all but five have a clause in their Constitutions requiring or permitting a census. Of these, twenty-eight take a census at intervals of ten years; one in eight; one in seven; one in six; and one in from four to eight

The oldest provision for a stated census, by State authority, is found in Pennsylvania in 1776 and New York in 1777, where, under the first Constitutions a septennial return of the classes represented was provided for. Since 1825 the census of New York has been at ten-year intervals, and for the last two periods (1855 and 1865) the schedules of inquiry have been more extended, and the deductions more thorough and systematic, than any hitherto published by State authority. As these two returns were made under my direction in every stage of progress, from the preparation of blanks to the distribution of the printed volumes, I shall have repeated occasion to refer to the operations and methods there employed, and to state some of the results of experience derived from these labors.

It will be convenient to notice our subject under three principal heads, viz.:

1st. As to the time fixed or allowed for making the enumeration.

2d. As to the blanks used, and subjects of inquiry.

3d. As to the agencies employed, and mode of compensation.

(1.) The earlier American census returns had no fixed point of time to which they referred, being taken on a given year. The first four census returns under the present National Constitution, were made with reference to August 1st. The last four with reference to June 1st of the census year. They are now returnable on or before the first of November. In New York it is also taken with reference to the first of June, and one month is allowed. In Wisconsin, from the same day, and two months are allowed; and in most of the other States, where a census is taken, the time fixed is the first of June or first of July.

The British census of 1861 was taken on the 8th of April, and referred to the population on the night of April 7th. Two or three previous returns in that kingdom were made also on a single day, but at different periods of the year. A want of

years. But a census is not actually taken in every State whose Constitution says it may be done. In eighteen States, the results of the National census become the basis of State equalization for their Legislatures. In Arkansas this will be done in 1880 and after.

uniformity in this regard is much to be regretted; and the summer season has this grave objection, that many persons, especially those in easy circumstances, are then temporarily away from their proper homes. It is probable that no date could be selected preferable to January 1, or at least early in that month, could other details be arranged properly. At this period of the year most families are together, and the results of the year previous are most easily obtained. Business accounts are then usually posted, and the earnings of fields and factories are fully known. More than all this, it is a season of leisure, and with proper announcements through the press, people would be able to prepare answers in readiness when called for. The adverse reasons against this time of year would be inclement weather and short days. The first of March would be the date next preferable, as the days are then of average length people are at home, the removals of residence have generally not been made, and it is a time of leisure with farmers.

In the British census as now taken, in several of the English colonies, and in some countries of Continental Europe, the census is taken in a single day, by immensely increasing the number of agents employed as Enumerators, and carefully preparing for the labor of that day. The only instance of an attempt to thus shorten the time for taking a census in this country, was in a census of the District of Columbia on the 11th of November last, taken under the auspices of the Department of Education, by authority of Congress, and of the local authorities. In this instance, through the agency of the Metropolitan Police, a most thorough and satisfactory result was obtained. A large majority of the returns were made on the first day, and the whole within four or five days from the one appointed, and so far as I have been able to ascertain, no census has ever yet been attempted where the last returns did not linger behind the period fixed by law for final completion.

In the highly and justly praised census of Great Britain, and with the facilities afforded by a well organized Registration system, the first preliminary statement of results were not laid before Parliament until two months after the census day.

It is, for obvious reasons, desirable to lessen the period of enumeration as much as possible, by a careful arrangement of

nearly equal districts; but it should never be forgotten that this wide distribution of appointments also increases the chances of incompetent Enumerators, more especially where the whole system must be extemporized for the occasion, as in the United States, and where so wide a diversity of subdivisions prevails in the different States. The township system in one, counties only in another, and the great range of subdivisions found in cities and wards, boroughs, precincts, parishes, districts, etc., presents a problem full of difficulties when attempting to adopt one plan of subdivision adapted to all. It is worthy of remark, that our system of public surveys and sectional subdivisions in the newer portions of our Union, afford precise and convenient designations for census districts.

(2.) As to the blanks used and the subjects of inquiry. We may classify the schedules hitherto employed in the census as follows:

1st. General blanks for family names.

2d. General blanks for individual names.

3d. Householder's schedules.

4th. Special blanks.

The earliest blanks used in taking the census allowed one horizontal line for a family, and the several columns were arranged for entering the number of persons in each, and such limited information relative to color, sex, age, occupation, etc., as this crude method would allow. It is obvious that no deductions concerning age, place of birth, occupation etc., could be attempted from these returns, except so far as provided for by separate columns, and that in order, in the remotest degree, to approach the standard of modern science in these details, the blank would need an immense horizontal extent. In fact, the United States census of 1840, the last one attempted by our government on this plan, provided headings extending through several yards of columns for entering these varied items, and finally obtained but a crude result at last.* In several of the States, as in Michigan, Wisconsin, etc.,.this primitive method is still practiced.

*The blanks of 1840, actually contained seventy-two columns for personal inquiry, thirty for mines, twenty-seven for agriculture, four for horticulture, eleven for commerce (including "butchers"), seven for fisheries, six for products of the forest, and one hundred and twenty-eight for manufactures. Total 285. Allowing half an inch of width as an average, it would reach nearly across twelve feet of space.

(2.) *General blanks for individual names.* This greatly improved plan of arrangement employed in 1850 and 1860 by the Federal Government, and now commonly adopted, provides a horizontal line across the sheet for each name in every family. In the various columns are entered the specific sex, color, age, civil condition, place of birth, occupation, etc., of the person as given by a member of the family to the Enumerator. The adoption of this plan constituted a prominent era in the history of statistics; and the student of social science, or political economy, who attempts to obtain comparable and satisfactory data of earlier periods, will find nothing but uncertain and general statements. The answers being reported in precise terms, allows this form of blank every degree of perfection that can be conceived of or desired in a census.

(3.) *Householder's schedules.* This form of blank, like the last, is arranged for entering the details relative to each individual, but is limited in size to the wants of single families, is left to be filled out by the head of each family, to be delivered on the day appointed for the census to the persons serving as Enumerators, and should contain printed instructions, with examples of filling up adapted to common understanding. It is the form used in the census of Great Britain, and most British colonies, and the kind used in the census of the District of Columbia already noticed.

(4.) *Special blanks*, adapted to particular inquiries, and varied to meet the peculiar nature of the subject to which they apply. These blanks were first employed on an extensive plan in the census of New York, in 1865, and with great success. They were of twelve varieties, and applied to the most common and generally distributed manufactories and institutions, concerning which details were required.

With reference to the inquiries that should be included in a census, an unfortunate tendency has prevailed of loading the columns with questions upon a wide range of subjects, by which the attention of the Enumerators has been distracted, and their labors prolonged; and there can be no question but that the accuracy of results has been seriously impaired from this cause.

The British census is a return of the population and nothing

else. The people are reported in the varied conditions of sex, color, age, nativity, civil condition, occupation, religion, etc., but the products of agriculture and manufactures, the statistics of churches, schools, newspapers, public institutions, etc., are left to be ascertained by other methods of inquiry, and are very fully set forth in their proper place. But with us all these and more are crowded into the decennial census; but in a vast number of instances but partially returned or wholly overlooked. While there may yet, for a season, be good reasons for retaining these questions, the tendency should be to gradually find other agencies to which to refer all extraneous labors. Under this view, in arranging the blanks of the New York census of 1855 and 1865, I left statistics of schools and academies to the Department of Public Instruction, and the Regents of the University; the carrying trade to the existing agencies of the Canal and Railroad systems; of insurance and banking to their proper Departments; and of incorporated institutions to the stated means of report provided in their charters.

With reference to the personal census, the blanks to be complete should embrace every name, with a precise return of color, sex, age in nearest years, or if under one year in months; civil condition, county and State, or particular country where born, occupation or means of support, whether a voter or an alien, whether deaf and dumb, blind, insane or idiots, with indications of the causes if known, and degree of education, at least so far as whether able to read or write. It should also return orphans under fifteen years of age, and *should not* attempt to get, by personal inquiry, any statistics as to pauperism or crime. These relate to disreputable classes, usually so considered, and attempts at concealment would impair results. Get these facts if desired, from official records, and these only.

I have observed that the people will generally answer the inquiries of the census cheerfully, fully and correctly, if they understand them. Exceptions to this rule are only found where the consequences of the return may affect the personal liability of the informant. The last census of New York for 1865, and of other States for the same year, were impaired in value by evasive answers relative to ages of males, through the fear of a military draft; and had the war continued a few

weeks longer, it would have been altogether impossible to make the enumeration, notwithstanding the imperative terms of the Constitution. The inquiries concerning ages and nativities, which the census includes, are too nearly like military enrolments to be distinguished by the common classes in times of war, and resistance against the unsupported labors of the Enumerators would surely attend their efforts. But in times of peace I apprehend that evasion or refusal may be considered so rare as to scarcely impair the general accuracy of results. Happily we have no cause of trouble in this regard in prospect, and with growing intelligence among the people the jealousies alluded to will pass away.

With reference to agricultural statistics, the ideal of perfection is realized in the French cadastral surveys, in which the soil, its contour, and the uses to which it is annually applied may be known. With us we can only hope to realize trustworthy results through a well organized system of State and county agricultural societies, reporting to a central Department, and provided with authority and means for obtaining annual statements of cultivation and products. One competent man in every school district could be found willing, for a small sum, to devote a day or two to this labor. The experiment has been tried by the New York State Agricultural Society, but, except in cases where particular efforts were made, it proved a dead failure, and for the best of reasons: it paid nothing for the labor, and had no penalties for neglect. Even the long delayed volume of Transactions promised to all who reported, was received by but few; and the attempt has been nearly or quite abandoned.

A passing word with reference to blanks for agricultural statistics:—They have usually been on large sheets with separate columns for each of the principal crops; their area and quantities, number and value of domestic animals kept for farm purposes, and sometimes blanks for unenumerated crops or products. To be in the least degree complete, these columns were necessarily multiplied greatly, and in the New York census of 1865 extended to 121 in number. The answers to these inquiries were necessarily given in a hurried manner and by general estimates in most cases. Consider, for a moment, the

superior advantage of using a sheet not larger than a letter page, to be left with the farmer to be filled by him at his leisure, and called for a day or two afterwards. On the face of this sheet there should be a printed form with name of person reporting, the town, county or district, number of acres improved and unimproved, estimated cash value of land and of buildings, and then a blank sufficiently wide to enter for each crop the acres sown and bushels harvested, and cash value, with blanks for entering the number and value of farm stock, and any farther information that might be required. On the back of the blank could be printed full instructions, and an example of filling up.

I believe that in most instances such a blank would be cheerfully and carefully filled up by the farmers themselves, and that the saving of time would overpay the extra cost. In condensing these blanks for publication, the quantities and values would need to be posted off into columns; but this, under the discipline of a well ordered office, would be rapidly and correctly done, and the columns thus prepared would be added with much less trouble than the straggling, crooked, and often defective columns of the present census blank.

As to manufacturing statistics, the official returns of our revenue system are supposed to give accurate data of some. The associated interest of manufacturers of iron, leather, lumber, etc., have, in some instances, led to inquiries and reports embracing more systematic and accurate statements than can ever be obtained through the agencies of a census; and we believe that there is more hope of improvement in the voluntary efforts of the parties interested in knowing the extent and distribution of their own particular branches of industry, than in anything that can be gained through a census. Instead of the general blanks hitherto chiefly used, special blanks, on the plan of those used in New York in 1865, might be employed to great advantage, and for other branches not thus provided for, a sheet of printed forms and instructions analogous to that suggested for agriculture might be employed.

But until some better disposition can be made of the statistics of agriculture and manufactures, they must be carried by the census as best may be. With suitable penalties for neglect

or refusal to answer, and sufficient intelligence to understand that there is no taxation behind them, the question is simply one of time and expense.

As to personal returns, the changes wrought by the recent war require that certain inquiries should be made with reference to the conditions of those made free by the success of the Union arms. By a single narrow column we could designate, opposite the name of every colored person, whether free before the war or freed by the war. There should also be an inquiry, applicable to all classes and everywhere, as to civil condition, and whether voter or alien. The census of the District of Columbia of November last had columns to designate the ancestry or parentage of each person; years resident in the District; whether permanent residence was intended; whether an owner or renter of real estate in the District; whether in the employment of the United States; and if so, whether civil, military or naval; name of school and months attending (if a scholar) during the past year; and the number of orphans, in addition to the usual questions of the census.

A characteristic feature of the English census is an inquiry relative to the religious creed or faith of individuals, and this has been copied in Canada, Nova Scotia and other colonies. But in this country I would consider such a question as quite out of place in a census, and that the least approach to the delicate subjects of religious faith or political creed should be entirely excluded from the schedules; and still more, that the agents employed should be instructed to avoid even the appearance of seeking information upon these subjects, in which it is the high privilege of the American citizen to hold his own opinions, and his reasons therefor, exempt from inquiry, and free from accountability.*

The concurrent experience of all who have had the charge of census labors would doubtless confirm this statement, that no attempt should be made by the Enumerators to sum up or classify their own labors, unless in a general total of per-

* Since this paper was read, a census of the city of Baltimore was attempted on the plan of Householders' Schedules. But an unfortunate question as to "Religion," ruined the whole. The returns are said to have brought in more than sixty thousand short of the real population. The work was thrown away, and a new enumeration on the rudest plan of primitive inquiry as to numbers, without any blanks of any kind besides common pass books for totals, was substituted.

sons, for early announcement of proximate results. In the earlier returns of New York the footings by towns were reported to the county clerk, who prepared a table for his county and forwarded this to the Secretary of State for publication. The work thus came together finished, but so full of discrepancies as to justify the most positive belief in errors, while there existed no means of correcting them.

We now come to notice the agencies employed and mode of compensation. In England, as already noticed, the system of registration of births, deaths and marriages, constantly organized and in operation, and kindred in purposes, affords a valuable medium for census purposes. Of registrar's districts in 1861, in England and Wales, there were 634. These were divided into 2194 subdistricts, and these again into 30329 enumeration districts. In the first two of these there were permanent officers who knew their districts, and the proper persons to employ, and who could lay out the enumeration districts carefully, and very nearly equal in amount of labors to be done. In towns, 200 houses, and in rural districts fifteen miles of travel were assigned to each person; and special provision was made for reporting those living on vessels; the absent marine; the royal navy, and persons employed in navigating the inland waters of the kingdom.

Our experience of the householder's schedule and one-day system in the District of Columbia was not strictly based upon an equal division of territory, as in most cases the members of police took the census of their customary patrol beats. These were smaller around the centres of trade and travel, and near markets, theatres etc., than in the quiet and respectable quarters of the better classes of inhabitants; and their attention was, in some instances, partially devoted to other duties. They were, however, able very readily to collect blanks from 200 houses in a day. But in the thinly settled country outside of the cities, although our men were mounted, their labors lingered longer, and were more difficult than in densely settled localities. I would think ten miles a day sufficient for one man to do thoroughly, where he had only to collect the blanks previously distributed. In New York, we had 1744 Enumerators in 1855, and 2332 in 1865, besides those appointed to In-

dian reservations. Their labors varied from 100 to 10000; and when all the data were written by the agent at the time of his visit, if he reported 300 names he did a full day's work. This is on the supposition of a concurrence of favorable circumstances; a dense population of the common or lower classes, with no time lost in travel, and none in delay. It would require but little experience to ascertain that at least five times as many persons could be reported in one day, in a dense crowded quarter than in a fashionable street, where delays at the door—the master at his business—the lady at her fashionable calls—"not at home"—"call again," and a dozen other excuses, delay the work of enumeration. But where Agricultural or Industrial statistics also require attention, the work is farther delayed. On a general average probably fifty or sixty names would be found a day's work.

In some of the States the census is reported by the assessors. I consider this highly objectionable, because it associates with the enumeration a suspicion of taxation, which will tend to impair the result. It is a well established principle in this class of statistics, that the inquiries, by whomever and however made, should be as much as possible independent, and understood to be only as a measure undertaken for procuring correct information for the general purposes of government. Hence the proposition for the employment of Internal Revenue Officers for taking the census is of all others, the most objectionable. These officials have hitherto never made a call to ask a question, unless it was soon *followed by a tax*. Evasion and concealment would be the inevitable result.

In the general government the agents have been hitherto employed under the marshals of the District Courts, and have been usually called "Census Marshals." But the memory of "Provost Marshals" has made the term so suspicious in the minds of the masses of our population that a change is imperative. The term "Enumerators" has been in many cases used, and as it correctly describes the duties of the office it should hereafter be employed.

Two modes of payment have hitherto been adopted. In most of our State and local census systems a *per diem* allowance, fixed by law and sustained by affidavits of service, has

been given from the State or county treasuries, upon the audit of supervisors or other financial officers. In New York, as now fixed by law, Enumerators are allowed $3.00 a day for the time actually and necessarily employed in taking the census and making one copy. The duplicate copy, reported to the Secretary of State, was, in most cases, neatly and handsomely prepared, and often by a female hand. Payment is made to depend upon acceptance at the central office; and this fact proved of the highest value by impressing an abiding sense of responsibility upon all, while it afforded a convenient means of enforcing attention to the revision of errors and the filling of omissions.

In the general census, a *per capita* rate is allowed for population returns, a specific rate for each manufactory, and for areas of agricultural districts reported.

It is but too well known that shameful frauds have been hitherto practiced under this method, where fictitious names could be multiplied in the large cities and in densely settled localities, with very little risk of detection, and it is suspected that an effort to secure unjust claims to representation in Congress, may have been connected with these frauds. Viewed in every light, the *per capita* plan of payment, appears to me more objectionable than a fixed allowance for the time employed.

But of all contrivances for covering fraud, under the cloak of science, the law of 1850 (still in force until changed), relating to the pay *for mileage* is the most ingenious. It gives ten cents a mile, to be ascertained "by multiplying the square root of the number of dwellings by the square root of the number of square miles in a district." Let us examine the operation of this highly "learned" method:

Of the series, ...	1, 4, 9, 16, 25, 100, 10 000, 1 000 000, etc.
The square roots are, ...	1, 2, 3, 4, 5, 10, 100, 1 000, etc.

in which it is apparent, that the relative proportion of the root to the number itself, rapidly diminishes at first and goes on forever approaching zero without ever reaching it. In short, the curve is a *Parabola.*

If an Assistant were assigned a county as his district, he would be justified in seeking for a description of its boundaries in the statutes. These extend the boundaries of all the

counties of New York, adjacent to Lakes Erie and Ontario, to the State line in the middle of the lake. I believe this is true of all the States bordering the great lakes, in which there would be found the legal sanction for claiming the benefit of mileage for the areas under water. But on shore, and especially in thinly settled States, the injustice of this rule becomes striking. When reduced to *per capita* rates, it allows twenty-six mills in Hamilton Co., N. Y., six mills in Herkimer Co., and four mills in Oneida Co. The factor of square root of square miles for Texas, is nearly twice that of all New England together, while its total population is not a fifth part as great.

Again, the rule of compensation to Marshals, for supervising the census, is absurd. If the district be under a million, it is at the rate of $1.25 per 1 000. If it exceeds a million, the rate from the beginning is $1.00 per 1 000. If he reports 800 000, he gets the same as for 1 000 000. For one *less* than a million he gets $125.00 more than for one *more* than a million.

The various intermediate agencies between the Enumerators and the central office should be suitably paid for services rendered, and at least one competent, active and honorable man should be assigned to each Congressional district, to select competent Enumerators, and exercise a vigilant supervision over them.

In every case, a careful revision and inspection should precede payment, and the central office should have power to withhold compensation until all needful corrections or deficiences were supplied.

With a suitable organization, a careful selection of a local superintendent in each Congressional district, and a like selection by the latter of persons, chosen on account of their intelligence and integrity, in every township, or, still better, in every election precinct or school district; with sufficient time to instruct every man uniformly in his duties, and a set of blanks, precise and simple in form, for their use, we might reasonably hope that our national census, upon which such mighty questions of public policy and private enterprise depend, could attain a fair degree of accuracy and be entitled to our fullest confidence.

I have observed that the character of returns from a given county or ward, have a general similarity as to fullness and

care, or the opposite; and that these characteristics depend much upon the character of the men by whom nominations of Enumerators were made. In large districts men were recommended for the office highly qualified, diligent and punctual in their labors, while in other cases, the class selected proved to be illiterate and incompetent as a class. In the former case the lists were prepared under an impressive sense of duty, and with an honest desire to serve the public interests, while in the latter the opportunity was seized upon to reward partizan services, or to promote some personal object or political intrigue. This experience has its moral, and should teach a lesson of wisdom, if we would arrive at a trustworthy result.

Statistical labors judiciously conducted, involve two separate processes, which cannot be carried on together without risk of error and loss of time. The recording of facts, with careful attention to their accuracy, constitutes the first division of these labors, and to these we have limited ourselves in this paper. The condensation and preparation of these facts for use, involving the three principal operations of tallying, posting and adding, belong to the office labors, and are so entirely within control that errors are needless and unpardonable. In many cases the work if correct will balance itself in the general result, and where this mode of proof is not practicable every process should be revised before being checked as right. A rule allowing a fixed sum to be deducted from the wages of a clerk committing an error to be paid to the one who detects it on revision, would check careless habits and secure vigilant attention. The labors of classification may in many cases be simplified, and mechanical devices are applicable in tallying and adding numbers, that would greatly facilitate clerical labor and relieve the clerk from that constant mental labor so exhausting when continued daily for long periods.

A daily report of work done by each clerk and a system of precise responsibility by which an error, if found, can be traced to its author beyond doubt, would constitute an essential feature in a well regulated office.

The classification should always have reference to comparability, with similar data of other period or locality, and if new subdivisions are adopted they should be so arranged that by their combination they would readily admit of such comparison.

11. Remarks on the Secular Variations of the Planetary Orbits. By J. N. Stockwell, of Brecksville, Ohio.

The secular variations of the elements of the planetary orbits, is one of the most interesting and important problems which the theory of the universal gravitation of matter presented for the consideration of mathematicians. This problem is one of immense difficulty, for, in order to determine the variation of the elements of a single planet, it is necessary to know the simultaneous variations of the elements of all the others. It is this circumstance which invests the problem with its greatest difficulty, and renders its rigorous solution one of the most valuable contributions to celestial mechanics.

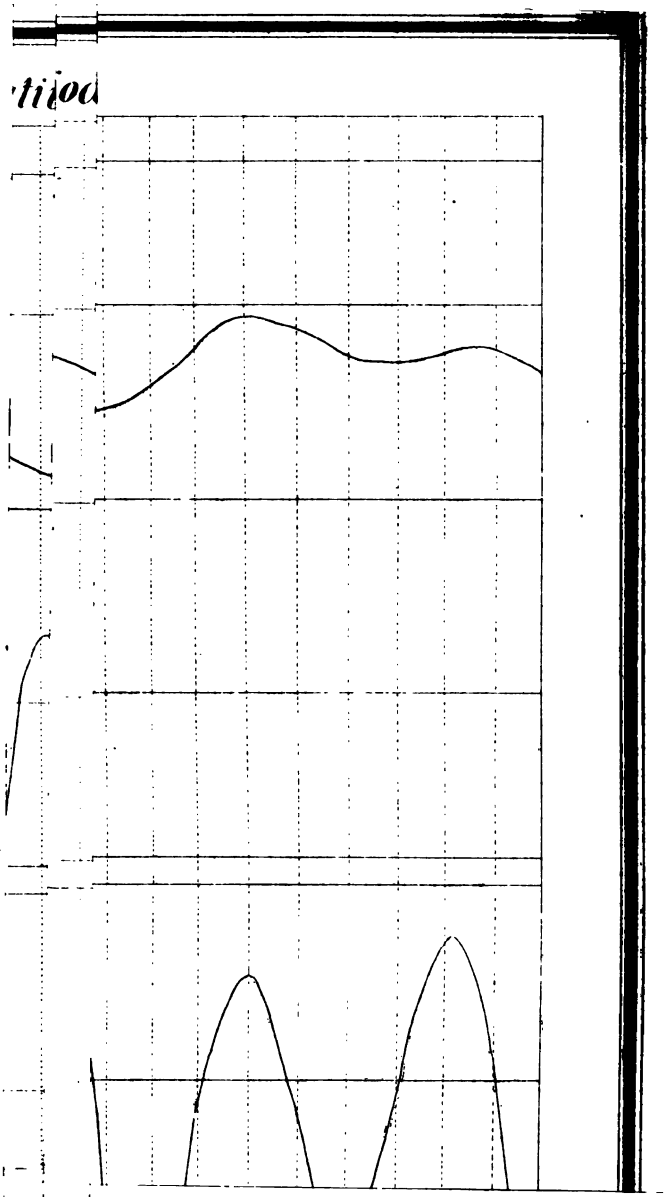
The researches of La Grange and La Place, towards the close of the last century, made known the laws which regulate and control the forms and positions of the planetary orbits; and these laws are embodied in several theorems of great elegance and generality. It was shown that all the variations of the elements of the planetary orbits, are confined within narrow limits; and that the stability of the planetary system is maintained by the forces of gravitation. The analytical investigation demonstrated the stability of the system; but did not, of course, make known the limits of variation within which each element would be confined; since the actual variations depend upon the relative values of the masses of the different planets of the system. It was therefore necessary, in order to determine the actual condition of the planetary system, during all past and future ages, to substitute the values of the masses and elements corresponding to each planet, in the analytical formulæ, and by means of a laborious process of elimination determine the particular values of the constants corresponding to each planet. The number of constants to be thus determined, amounts to eighty, for the determination of the eccentricities and perihelia of the eight principal planets; and eighty more are required for the determination of the nodes and inclinations. A knowledge of the actual condition of the planetary system at any period of time, therefore, requires us to determine the values of one hundred and sixty constant quantities,—thirty-two of which are indeterminate by analysis,

but may be derived from the values of the elements at a given epoch.

La Grange made the first successful attack on the problem of finding the approximate numerical values of the constants which determine the variations of the elements of the seven principal planets which are within the orbit of Neptune. But his solution was based on values of the masses of the planets which differed much from the truth. His determination of these constants could therefore be considered only as provisional, and authoritative only until more correct values of the masses of the planets should be arrived at. About half a century from the time of La Grange's investigation, when more accurate values of the planetary masses had been obtained, Le Verrier undertook a new determination of these constants. He very skilfully determined all the constants, together with the coefficients of any supposed errors in the masses of the planets. But his determination of these coefficients was founded on two theorems which are true only on the supposition that the assumed masses are extremely near the truth; or, in other words, on the supposition that the possible errors of the masses are infinitely small. This investigation of Le Verrier's was made about thirty years ago; and the values of the masses which he used, are some of them, considerably different from the values received at the present time. These successive corrections to the masses, together with some small corrections to the elements of the orbits, taken in connection with the discovery of the planet *Neptune*, was a sufficient incentive for submitting the question to a new discussion. I therefore, some ten years since commenced the investigation, using as the groundwork of my solution the masses and elements of the orbits of which are used in the construction of the *American Ephemeris and Nautical Almanac*, with the exception of the mass of the *earth* and the elements of *Neptune;* and have since devoted such time as I could spare from other duties, to the prosecution of this work, which is now substantially completed.

I purpose now to give a brief abstract of the results of my investigation.

Since I completed the determination of the constants, I have



I

computed the eccentricities and places of the perihelia, of the planets *Mercury* and the *Earth*, at intervals of 10 000 years, during a period of 2 000 000 years; and have drawn a chart exhibiting the relative eccentricities at all times during that period. The eccentricity of *Mercury's* orbit will always be confined within the limits of 0.231 994 and 0.123 274; the minimum being a little more than one-half of the maximum. But it is not probable that these extreme limits of eccentricity will ever be reached, since the coefficients of the *time*, in the expression of that element are incommensurable, and consequently the *co-sines* of all the angles can never be equal to ± 1, at the same time. During the 2 000 000 years which I have charted, the eccentricity has been confined within much narrower limits,—the greatest maxima being only 0.2072, or slightly greater than the present value of that element, and the smallest minima being 0.1586; so that the whole variation during 2 000 000 years amounts to only 0.0486. We see by the chart, that the variations of its eccentricity consists of a series of small elevations and depressions, having a comparatively short period; but these small elevations are at present combined so as to produce one grand and uniform swell of the eccentricity, during a period of 400 000 years.

The mean annual motion of *Mercury's* perihelion is 5″.509-545. It therefore performs a complete revolution in the heavens, in about 235 200 years. And the greatest quantity by which the perihelion can differ from its mean place amounts to only 17°.

The superior limits of the eccentricity of the orbits of the planets *Venus*, the *Earth* and *Mars*, are respectively 0.073 930, 0.0 693 888 and 0.1 402 156; and their inferior limits are each equal to nothing. The orbits of these three planets are the only ones in the solar system which can become circular through the agency of the forces of gravitation. The perihelia of these three planets can have no *mean motion*, since the ellipticity of the orbits is a vanishing element, and the position of the transverse axis becomes an indeterminate quantity the moment the ellipticity disappears. It is therefore possible for a new ellipticity to commence, with its perihelion diametrically opposite the point at which it disappeared; and in such a

case the transverse axis would apparently revolve with an infinite velocity.

The eccentricity of the earth's orbit is one of the most interesting and important elements of astronomy. Not only does it have an important influence in modifying the amount of light and heat received from the sun in the course of each year, but it also has an important bearing in matters pertaining to the chronology and antiquity of the human race. For the inequalities which that element produces in the sun's apparent motion are so great as to force themselves upon the notice of observers, as soon as they pass from a barbarous to a civilized and enlightened state. And when an inequality is once detected it is carefully observed, and its maximum value accurately ascertained and recorded. But since the eccentricity is itself a variable quantity, the inequalities which it produces in the sun's apparent motion will be different at different periods of time; and no two observers will obtain the same values of these inequalities unless they are exactly, or very nearly, contemporaneous. The values of these inequalities, as recorded in the annals of a nation, therefore, serve as an accurate chronological record of the epoch of the observations on which the recorded inequalities are founded. If we, therefore, know the values of these inequalities at two different epochs, we may use them to determine the interval of time between them, when we know the exact law according to which the eccentricity varies. On the other hand, when the inequalities and epochs are both known, we may use them to correct any assumed law of variation of the eccentricity.

As the question relating to the antiquity of man, has been so thoroughly ventilated by several members of this Association, from the study of the fossils of prehistoric ages, any farther allusion to the subject at this meeting might seem improper and out of place. But I trust you will pardon me for alluding, not simply to the fact of his existence during many centuries antecedent to the epoch of his creation, according to the usually received chronology, but to the fact that he had made great advances in civilization and science, at an epoch antedating European records, and the heroic ages of Greece and Rome, by more than two thousand years. These records

of his civilization and scientific culture, I need scarcely say, are based on astronomical elements of a former age,—elements which are perpetually changing. And it will be one of the grandest triumphs of astronomy, to be able to reconstruct a chronology which the lapse of ages has in great measure obscured.

About a century ago the French astronomer, Legentil, was sent to India to observe the transit of *Venus* over the sun. He there became acquainted with a learned Brahmin, and from him obtained a set of tables and a knowledge of Hindoo astronomy. The astronomical tables he carried with him to France, and they were subsequently published in the Memoirs of the Academy of Sciences of Paris. These tables contained the various astronomical elements which are necessary in order to calculate the positions of the heavenly bodies, at an epoch 3102 years before the Christian Era. The great antiquity of the reputed epoch of these tables induced astronomers to reconstruct tables for that epoch, by applying to the existing elements the changes which should have taken place since that epoch, according to the theory of universal gravitation. The identity of the resulting elements, when thus corrected, was not regarded by scientific men as sufficiently close to warrant the conclusion that they were founded on the state of the heavens at that epoch; and they were therefore regarded as mere fanciful creations of the imaginative Hindoos.

But a question of so much interest and importance ought not to be hastily decided. All the circumstances which can possibly have an intimate or remote bearing on the question should be carefully considered, and due weight allowed them in the final summing up of evidence.

At the time of the discovery of these tables the development of the theory of universal gravitation was in its infancy; —the simple elements of the gravitating forces were very imperfectly known; and consequently the elements of the planetary motions at that time, when thus corrected for the effects of the gravitating forces during a period of nearly five thousand years, would very imperfectly represent the original elements. But during the present century the theories and elements of astronomy have been very greatly improved; and

we are now in possession of the means of obtaining the variations of all the elements of the planetary motions, during all past ages, more perfectly than would have been possible for our predecessors. And if our elements of astronomy, as they increase in precision, represent more and more perfectly, the elements recorded in these ancient tables, the presumption seems legitimate that the recorded elements are founded on the observed state of the heavens at that epoch. But if, on the other hand, they differ more and more widely from the recorded elements, the conclusion seems alike inevitable that they are not founded on fact, but are merely the idle fancies of the people by whom they are entertained.

We will now apply the test we have indicated to two of the astronomical elements which are given in the *Tirvalore Tables*, as they are technically called. These elements are the equation of the sun's centre (which depends on the eccentricity of the earth's orbit), and the obliquity of the ecliptic. We have chosen these two elements for comparison, because they may be readily determined by observation, and are alike independent of all knowledge of the true theory of astronomy. According to these tables, the epoch of which is assumed to be 4 952 years before the year 1850, the equation of the sun's centre was 2° 10′ 32″. And a writer in the *Encyclopedia Brittanica*, states that the equation of the sun's centre, at that epoch, should be 2° 6′ 28″, according to the formula of La Grange. This differs from the recorded value by only 4′ 4″. LaPlace's formula would diminish this difference by 30″. But according to my determination of the eccentricity, which is based on the latest values of the masses of the planets, the value of the equation at that time was 2° 7′ 24″, which differs from the recorded value by only 3′ 8″. According to the *tables* the obliquity of the ecliptic at the same epoch was 24°. LaGrange's formula gives 23° 51′ 13″, which differs from the recorded value by only 8′ 47″. LaPlace's formula gives 24° 5′ 50″ for the obliquity, which exceeds the recorded value by only 5′ 50″. And my own computation gives the obliquity at that time equal to 24° 3′ 8″, which exceeds the recorded value by only 3′ 8″.

According to this comparison the *tables* endure the test

which we proposed to apply; and we may therefore regard the question of their authenticity as still an open one. I hope to be able, at an early day, to extend the comparison to the various other astronomical elements; and should the evidence continue to be cumulative, the conclusion will be inevitable that they were founded on observation.

The chart of the eccentricity of the earth's orbit may serve to give a general idea of the variations of the eccentricities of the orbits of *Venus* and *Mars*, although the latter would be subject to greater variations. A simple inspection of the chart shows that the average time required for the eccentricity of the earth's orbit to pass from a maximum to a minimum value, is only 45 500 years. This time seems to be very nearly independent of the amount of change required, in order to obtain these conditions.

If we now pass to the consideration of the elements of the four largest planets of the system, *Jupiter*, *Saturn*, *Uranus* and *Neptune*, we shall notice some curious and interesting relations. These planets compose a system by themselves, which is practically independent of the other planets of the solar system.

The maximum and minimum limits of eccentricity of these four planets are as follows: —

	Maximum.	Minimum.
Jupiter,	0.0608274,	0.0254942.
Saturn,	0.0843298,	0.0123710.
Uranus,	0.0779496,	0.0117464.
Neptune,	0.0145058,	0.0055729.

The whole limit of variation of eccentricity in *Neptune's* orbit is therefore only about one-half the present value of the eccentricity of the earth's orbit.

But the most curious relation developed by my researches, pertains to the relative motions and positions of the perihelia of the different planets. The mean motion of *Jupiter's* perihelion is exactly equal to the mean motion of the perihelion of *Uranus;* and their mean longitudes differ by exactly 180°. The mean motion of *Saturn's* perihelion is very nearly six times that of *Jupiter* and *Uranus;* and this latter quantity is very nearly six times that of *Neptune;* or, more exactly, 985 times

the mean motion of *Jupiter's* perhelion is equal to 163 times that of *Saturn;* and 440 times the mean motion of *Neptune's* perihelion is equal to 73 times that of *Jupiter* and *Uranus.* The mean annual motions of the perihelia of these four planets are as follows; namely,

Jupiter and Uranus, ...	3″.716 923
Saturn, ...	22″.460 985
Neptune, ...	0″.616 686

The perihelion of *Saturn*, therefore, performs a complete revolution in the heavens in 57 700 years; that of *Jupiter* and *Uranus* in 348 676 years; while that of *Neptune* requires no less than 2 101 555 years to perform the circuit of the heavens.

We may observe that the law which controls the motions and positions of the perihelia of the orbits of *Jupiter* and *Uranus*, is of the utmost importance in relation to their mutual perturbations of *Saturn's* orbit. For in the existing arrangement the orbit of *Saturn* is affected only by the *difference* of the perturbations of *Jupiter* and *Uranus;* whereas, if the mean places of the perihelia of these two planets were the same, instead of differing by 180°, the orbit of *Saturn* would be affected by the sum of their disturbing forces. But notwithstanding this favoring condition, the elements of *Saturn's* orbit would be subject to very great perturbations from the superior action of *Jupiter*, were it not for the comparatively rapid motion of its perihelion; its equilibrium being maintained by the very act of perturbation. Indeed, the stability of *Saturn's* orbit depends almost entirely upon the rapidly varying positions of its transverse axis. For, if the motions of the perihelia of *Jupiter* and *Saturn* were very nearly the same, the action of *Jupiter* on the eccentricity of *Saturn's* orbit would be at its maximum value during very long periods of time, and thereby produce great and permanent changes in the value of that element. But in the existing conditions the rapid motion of *Saturn's* orbit prevents such an accumulation of perturbation, and any increase of eccentricity is soon changed into a corresponding diminution.

I said the mean angular distance between the perihelia of *Jupiter* and *Uranus* was exactly 180°. But the conditions of these motions are sufficiently elastic to allow of a considerable

deviation on each side of their mean positions. . In fact, the actual position of *Jupiter's* perihelion may be in advance of its mean place to the extent of 23° 54′; and it may fall behind its mean place to the extent of 22° 16′; while the perihelion of *Uranus*, may be in advance by 36° 26′, or behind its mean place to the extent of 45° 41′. It therefore follows that the nearest approach of the perihelia of these two planets will be 110° 25′, and their greatest distance apart will be 238° 42′. The amount by which these elements differ from the mean at present is only 21° 19′, or about one-third of their ultimate limit of deviation. This deviation from their mean places will continue to increase during a period of 35 000 years, when it will attain the value of 68°. They will then alternately approach and recede from their mean positions, at intervals of time during a period of 170 000 years, at which time they will be very near their mean positions,—the longitude of *Jupiter's* perihelion being 220° 14′, and that of *Uranus* being 36° 33′; and consequently the distance between them will be 176° 19′, which differs by only 3° 41′ from the mean value of that element.

The consideration of the phenomena depending on the variations of the nodes and inclinations will form a proper subject for another communication.

NOTE. In the accompanying Chart, the unit of abscissas is equivalent to 10 000 years; and the unit of ordinates is equivalent to 0.001, for the Earth's orbit, and to 0.003 for Mercury's orbit. The curve, therefore, represents the eccentricity of Mercury's orbit only one-third of the actual value relatively to that of the earth.

---

12. On Hansen's Theory of the Physical Constitution of the Moon. By Simon Newcomb, of Washington.

The great reputation of the author has given extensive currency to the hypothesis put forth by Professor Hansen some years since, that the centre of gravity of the moon is considerably farther removed from us than the centre of figure. The consequences of this hypothesis are developed in an elaborate mathematical memoir to be found in the twenty-fourth volume of the Memoirs of the Royal Astronomical Society. But the reception of the doctrine seems to have been based rather on

faith in its author, than on any critical examination of its logical foundation. Such an examination it is proposed to give it.

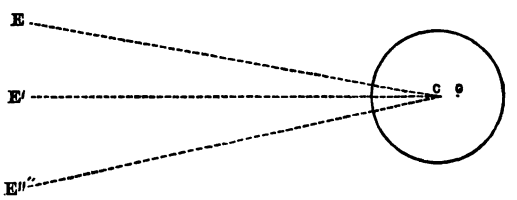
An indispensable preliminary to this examination is a clear understanding of what the basis of the doctrine is. Let us then consider these three propositions:

1. The moon revolves on her axis with a uniform motion equal to her mean motion around the earth.

2. Her motion around the earth is not uniform, but she is sometimes ahead of, and sometimes behind her mean place, owing both to the elliptic inequality of her motions and to perturbations.

3. Suppose her centre of gravity to be farther removed from us than her centre of figure, and so placed that when the moon is in her mean position in her orbit, the line joining these centres passes through the centre of the earth.

Let us also conceive that these two centres are visible to an observer on the earth. Then a consideration of the geometrical arrangements of the problem will make it clear that when the moon is ahead of her mean place the observer will see the two centres separated, the one nearest him being farther advanced in the orbit, while when the moon is behind her mean place the nearest centre will be behind the other. This apparent oscillation of the two centres is, indeed, an immediate effect of the moon's libration in longitude, as may be seen from the following figure in which the circle represents the moon, C and G her centres of figure and gravity, and E E′ E″ the



positions of an observer on the earth, relatively to the face of the moon, when she is behind, in, and ahead of her mean place.

Now the inequalities in the moon's motion computed from the theory of gravitation, are those of a supposed centre of gravity. But the inequalities given by observation are those of the centre of figure. Hence, in the case supposed, the

inequalities of observation will be greater than those of theory. Also their ratio will be inversely as that of the distances of the centres which they represent.

Professor Hansen, in comparing his theory with observations, found that the theoretical inequalities would agree better with observation when multiplied by the constant factor 1.0001544. Supposing that this result could be accounted for on the hypothesis of a separation of the centres of gravity and figure, he thence inferred that the hypothesis was true. But the result cannot be entirely accounted for in this way, because the largest inequality of theory (evection) has a factor (eccentricity) which can only be determined from observation, and therefore, even the theoretical evection is that of the centre of figure and not of the centre of gravity. It must not be forgotten, that the eccentricity, which is not given by theory, is subject to be multiplied by the same factor that multiplies the other inequalities.

To be more explicit:—

Let e be the true eccentricity of the orbit described by the moon's centre of gravity; then the true evection in the same orbit will be $e \times A$;

A being a factor depending principally on the mean motions of the sun and moon; and on Hansen's hypothesis the apparent evection, or that of the centre of figure, will be

$$e \times A \times 1.0001544.$$

On the same hypothesis, the eccentricity derived from observation, being half the coefficient of the principal term of the equation of the centre, will be

$$e \times 1.0001544,$$

and the theoretical evection computed with this eccentricity will be $e \times 1.0001544 \times A$,

which is the same with that derived from observation. Hence:

The theoretical evection will agree with that of observation notwithstanding a separation of the centres of gravity and figure of the moon.

That Hansen overlooked this point is to be attributed to his method of determining the lunar perturbations, by numerical computation from the various elements of the moon's motion, so that the manner in which the inequality depends on the

elements does not appear. It is only when we determine the perturbations in algebraic form that this dependence appears.

Passing now from the evection, the next great perturbation of the moon's motion is the variation. But the value of this perturbation has not been accurately determined from observation, because, attaining its maxima and minima in the moon's octants, it is complicated with the moon's semi-diameter, and parallactic inequality. Even if the semi-diameter is known, the two inequalities in question cannot be determined separately with precision, because their coefficients have the same sign in that part of the moon's orbit where nearly all the meridian observations are made. From this cause Airy's value of the parallactic inequality from all the Greenwich observations from 1750 to 1830 was 3″ in error. And when in his last investigation Airy rejected the observations previous to 1811, owing to some uncertainty as to what semi-diameter should be employed, the result was still a second too small. It is therefore interesting to find what value of the variation will result if we substitute the known value of the parallactic inequality in Airy's equations for the determination of that element. Neglecting those unknowns which have small coefficients, these equations are from 1806 to 1851,

1806 — 15	10.66 W +	28.14 V = +	17.2
16 — 24	9.45 +	30.92 +	24.9
25 — 33	9.43 +	29.26 +	42.1
34 — 42	9.29 +	27.28 +	10.8
43 — 51	9.05 +	23.36 +	7.9
Sum, . .	47.88 W +	138.96 V = +	102.9

In these equations W × 0″.73 represents the correction to the coefficient of variation, and V × 3″.77 that to the coefficient of parallactic inequality. We now know from recent special investigations that the latter coefficient is very near 125″.50. Airy's provisional one was 122″.10, whence

$$V = \frac{125''.50 - 122''.10}{3''.77} = 0.90$$

The sum of the preceding equations gives

$$W = 2.15 - 2.90\,V = -0.46.$$

The resulting correction to the provision variation (2370″.3)

is therefore $-0.46 \times 0''.73 = -0''.34$, making the variation derived from observation . . . . . 2369″.96,
while Hansen's theoretical value is . . . 2369 .86,
and Delaunay's . . . . . 2369 .74.
The differences are too minute to found any theory upon. Leaving the evection and variation, the other inequalities are so minute that their product by Hansen's coefficient is altogether insensible.

Summing up the results of our inquiry, it appears that in the case of the evection, the supposed discordance between theory and observation would not follow from Hansen's hypothesis, and, therefore, even if it exists, cannot be attributed to that hypothesis as a cause. In the case of the variation no such discordance has been proved. In the case of the other inequalities the discordance would be insensible. The hypothesis is therefore without logical foundation.

The question whether the evection given by observation is really greater than that deduced from theory, although it does not affect our conclusion, is yet interesting and important. It appears from the commencement of Hansen's computations in his "*Darlegung*,"* that his theoretical perturbations were computed with an eccentricity equal to .05490079, and that, on comparing with observation, he found that this eccentricity should be increased to .05490807, which is the value adopted in the *Tables de La Lune*, and is greater than that employed in his theory in the ratio 1 : 1.0001326. If, now, he had employed this corrected eccentricity to recompute the evection, the latter would have been increased in the same ratio, and the outstanding discrepency between theory and observation would have been reduced $.0001544 - .0001326 = .0000218$ of it value, or 0″.10, a quantity no larger than may be attributed to the errors of theory and observation.

It does not, therefore, appear that there is any sensible discordance between the values of the two great perturbations of the moon which result from Hansen's theory, and those which result from all the Greenwich observations from 1811 to 1851.

*Erste Abhandlung, § 4, pp. 173–175.

13. THE RESUSCITATION OF THE CINCINNATI OBSERVATORY. By Cleveland Abbe, of Cincinnati, Ohio.

As our Association is so deeply interested in, and identified with, the progress of the scientific institutions of the country, I take pleasure in presenting to the members the following brief notice of what is contemplated, and being accomplished, in the city of Cincinnati.

The Queen City of the West, so prominent as an early patron of science and learning, has, through those who represent its wealth and cultivation, resolved to revive the interest in the Astronomical Observatory, founded by the labors of Professor O. M. Mitchel in 1842, to enlarge its field of usefulness and to establish its future existence upon a firm foundation.

It is well known that the expense of building and equipping the Cincinnati Observatory was borne by over a thousand individual contributors of small sums of money, the chief benefactor being the late Nicholas Longworth, Esq., the donor of the present site upon Mt. Adams. The countenance of our own Association, and especially the cordial encouragement and timely assistance vouchsafed for many years by the late lamented Superintendent of the Coast Survey, contributed not a little to the prosperity of the Institution. Never having received any permanent endowment, however, it inevitably followed that on the removal of General Mitchel from Cincinnati, the interests of the Observatory were generally lost sight of. Thus it has happened that for the past ten years the corroding effects of time have become more and more plainly visible, while on the other hand the rapid growth of the city has surrounded the Observatory site by dwellings and factories, whence issue clouds of smoky vapors even in the hottest months of summer.

At a meeting of the Trustees of the Observatory, held last winter, the interests of the establishment were entrusted to me as Director, and I take pleasure in bringing to the notice of this Association the broad and enlightened policy that the Trustees have decided to adopt as being for the best interests of the Institution.

"Usefulness" being the criterion by which business men very properly judge of the value of an Institution, we shall

necessarily seek to advance not only science but also its practical applications. Accordingly it has been resolved that our complete ideal must embrace the field of activity indicated by the following articles:

1. In Astronomy, the application of large Equatoreals to the study of the heavens will enable us to further the science, properly so called. The establishment of a meridian transit and a vertical circle will hereafter follow, and will especially be necessary in connection with the Geodetic works to be mentioned.

2. In practical Astronomy, the determination of latitudes and longitudes, as the basis of accurate maps of the surrounding country, will claim a large share of our attention.

3. In connection with the latter, Hypsometry and Geodesy in its general expansion will be cultivated; combined herewith the conduct of a "Pendulum Survey" will be of great interest, especially if, as we expect to be able to do, the use of the Repsold Bessel Pendulum be introduced. Although an extended survey of the Ohio Valley will require a long series of years, yet it will ever be kept steadily in view as the appropriate work of the Observatory. The proposal to establish a special school of Geodesy, under the auspices of a University about to be inaugurated at Cincinnati, has met with a favorable consideration.

4. The Meteorology of our country is a matter of such vital importance to the national interests, and one so interesting to the astronomical observer, that it has been decided to establish a complete Meteorological Observatory, furnished with self-recording instruments, so soon as the proper funds are secured.

5. In direct connection with the latter it is considered desirable that there be published a daily meteorological bulletin, compiled from telegraphic dispatches from all parts of the Ohio Valley. For this, the central position of Cincinnati offers great advantages, and we shall expect to distribute this bulletin widely to those places where the knowledge it contains is desired.

6. It is considered that we shall not depart too widely from our province, if we include in our Institution the Magnetic Observatory, also; this will complete our equipment as an

Astronomical and Physical Observatory, and seems especially called for when we consider the fragmentary condition of our knowledge of the magnetic elements in the interior of this continent.

While the works here contemplated will demand a large force of men and instruments, they equally necessitate the removal of the Observatory to a new and more favorable location; this step having been at once decided upon it is our confident expectation that the removal will be effected within a year; therefore at the next meeting of the Association I hope to present a report of that which has been done, rather than of that which is contemplated.

---

14. PHOSPHORIC ACID, A CONSTITUENT OF BUTTER. By Professor E. N. Horsford, of Cambridge.

LIEBREICH* found some three years ago in the alcoholic extract of the brain, and also of nerve fibre, a crystalline substance of fatty character which readily broke up into several fatty bodies, including cerebrin, stearic, and perhaps other fat acids, and a body having the composition $C_{10}\ H_{14}\ NO$, which Liebreich called neurine, which was combined with glycerophosphoric acid.

This nitrogenous body—*neurine*—was observed by Wurtz to consist of the same elements in the same proportions that would be required to form an oxide of ammonium, in which one of the atoms of hydrogen is replaced by ethyl, and the other three by three atoms of methyl, thus:

$$N\left\{\begin{matrix} C_4\ H_5 \\ C_2\ H_3 \\ C_2\ H_3 \\ C_2\ H_3 \end{matrix}\right\} O = C_{10}\ H_{14}\ NO.$$

Bæyer and Liebreich found sugar in the compound, and the whole body yielded to analysis the constitution of

$$C_{222}\ H_{241}\ N_4\ O_{30}\ PO_5.$$

It is, possibly, the body formerly known as lecithin, and was called by Liebreich, *Protagon*.

*Liebig's Jahr's Bericht. 1865, p. 647.

Wurtz attempted the synthetic production of neurine, and succeeded perfectly so that in all its reactions the body he obtained corresponded with the neurine derived from the brain.

It combines with platinum and gold salts and with various acids yielding the same crystalline forms and the same reactions that were obtained with the neurine procured from the brain.

Since then, Hermann has found protagon in the blood corpuscles, and Hoppe-Seyler in the serum, and in the red and white corpuscles, in Indian corn, in wheat, in wine ferment, in the yellow of the egg, and in some fat oils.

In view of these facts it seemed highly probable that it would be found in butter, which is the fat constituent of milk, from which the blood and brain of the young mammals derive their nutriment.

In my first experiment in search of this body, or of the phosphoric acid it contains, I selected a very choice sample of butter, quite free from buttermilk and water, rubbed it up with carbonate of magnesia and pulverized caustic potassa. It saponified in a short time to a mealy mass. This mealy mass I threw, in successive small portions, into a platinum crucible and burned. The residuum I extracted with nitric acid and water, filtered off the unburned charcoal, and tested with molybdate of ammonia. The precipitate was so immediate and large, that I was led to suspect that either my potassa or magnesia contained phosphoric acid. They proved to be entirely free from it. It then occurred to me that a trace of caseine or of buttermilk present in the butter might be the source of the phosphoric acid.

I proceeded then to treat a mass of butter with ether, pouring off the extract when the residue was reduced to a pale milk-and-water fluid. The extract I left to spontaneous evaporation. The milk-and-water fluid yielded scarcely a trace of phosphoric acid to molybdate of ammonia.

From the ether extract on slight concentration, there separated abundant crystals, and at length the whole became, as the ether evaporated, a semi-fluid crystalline magma.

This mass I rubbed up with carbonate of magnesia and powdered hydrate of potassa, calcined as before, and dissolved

in diluted nitric acid, filtered through sand previously boiled with hydrochloric acid and thoroughly washed with water, and tested with molybdate of ammonia. The yellow precipitate was prompt and abundant.

There can, therefore, be no doubt of the presence of phosphoric acid in the etherial solution of this sample of butter. It is not improbable that it is present in all butter.

As butter has been found to contain:—

Butyric acid,	$C_8\ H_8\ O_4$	Palmitic acid,	$C_{32}\ H_{32}\ O_4$
Caproic "	$C_{12}\ H_{12}\ O_4$	Margaric "	$C_{34}\ H_{34}\ O_4$
Caprylic "	$C_{16}\ H_{16}\ O_4$	Stearic "	$C_{36}\ H_{36}\ O_4$
Capric "	$C_{20}\ H_{20}\ O_4$	Butic "	$C_{40}\ H_{40}\ O_4$
Myristic "	$C_{28}\ H_{28}\ O_4$		

glycerine and phosphoric acid, and, as Protagon has been found to contain glycerine, sugar, phosphoric acid, margaric acid, stearic and other fat acids, it is hazarding little to say that protagon or some nearly allied body will probably be found to be an important constituent of butter.

---

15. FLUORINE IN THE HUMAN BRAIN. By Professor E. N. Horsford, of Cambridge.

THE large percentage of phosphoric acid in the brain and nerves, amounting (according to v. Bibra, Bourgoyne, Fremy and Warren,*) to some 4.50 per cent. in the dried brain, suggests the possibility of finding fluorine in the same tissues, since fluorine is a very frequent concomitant of phosphoric acid in the mineral kingdom, and is found as fluoride of calcium in the teeth and bones. It might, moreover, very naturally be expected to be found in the brain, after it was found in the blood by George Wilson in 1846, and since then by Nicklés in albumen, gelatine, hair, blood and urine.

The specimen of brain which I employed in my analysis was one long kept in alcohol, but from which, through neglect, all the alcohol had evaporated, and there remained a compact, corrugated mass, of a texture somewhat like that of a moderately hard pine-apple cheese. Pieces of this mass, ground up

*From unpublished manuscript notes supplied to the author by Professor C. M. Warren.

with caustic potassa or lime, gave readily the indications of phosphoric acid, with nitric acid and molybdate of ammonia. The same reagents indicated phosphoric acid after digestion of portions of the brain with oil of vitriol.

The chief difficulty in obtaining trustworthy evidence of the presence of fluorine lies in the circumstance that the reagents employed in the reaction may sometimes, indeed do oftentimes, contain fluorine.

My lime was prepared by carefully elutriating water-slacked quick lime, containing considerable silicate of lime and a trace of phosphoric acid, and subsequent ignition of the dried deposit. This gave a substance of extreme fineness. Silica was prepared from common water glass, by separation with hydrochloric acid, long continued washing and drying. These two ingredients, with oil of vitriol from my laboratory, mixed with silica, in all respects as in the last experiment, except that the brain was not present, gave no indication of fluorine.

With these reagents I proceeded to test the dried brain for fluorine. A portion of it was ground up with the dried lime and calcined on platinum, the hydrogen and carbonaceous matter burning off with great facility.

The calcined mass was then ground with silica, prepared from the hydrate and mixed with the oil of vitriol in the bottom of a test tube. A tube, moistened in the interior, was inserted through a cork into the test tube, and the test tube and its mixed brain ash, lime, silica and sulphuric acid, gently heated. The fumes that arose condensed in part on the upper portions of the test tube, and along the moistened interior of the inserted tube, forming a white film which was non-volatile before the blow-pipe. This was *silica* derived from the *hydro-fluo-silicic* acid, evolved from the mixture, and proof of the presence of *fluorine*.

Another sample of the brain was rubbed up with powdered hydrate of potassa and calcined magnesia to a paste calcined in platinum, saturated with water glass, again heated to drive off the water, powdered and mixed with oil of vitriol in a flask. The vertical eduction tube was of nearly half an inch calibre and four inches high, and connected with a smaller curved tube, which discharged under water. On applying heat to the

flask, there appeared a white powder along the moistened eduction pipe and the curved tube, and finally in the water, which latter readily dissolved in potassa liquor. The water glass, magnesia and potassa, as well as the sulphuric acid, were proved to be free from fluorine. This experiment was conclusive as to the presence of fluorine in the brain.

A similar experiment with the residue obtained from treating a portion of the brain for a series of days with successive portions of nitric acid and evaporating to dryness yielded a similar result, though, for some reason, not so marked as that obtained with the ash by burning the quick lime mixture, and also by the process with water glass.

---

16. On the Source of Free Hydrochloric Acid in the Gastric Juice. By Professor E. N. Horsford, of Cambridge.

The long disputed position of Prout, that the gastric juice contains free hydrochloric acid, was at length established by C. Schmidt, who in an absolute quantitative analysis of the juice found about twice as much hydrochloric acid as was required to neutralize all the bases present. The prolonged discussion of this subject now since 1823, has brought to light through the researches of Lassaigne, Tiedemann and Gmelin, Berzelius, Lehmann, Claude, Bernard, Blondlot and numerous others, the unmistakable evidence of the presence of lactic acid, and of acid-phosphates in the gastric juice, which might or might not be due to the presence of lactic acid or hydrochloric acid. A point of special interest to the chemist and physiologist, still remained, and was this:

How could free hydrochloric acid be secreted from the blood which is an alkaline fluid? This question was submitted to experiment with entirely satisfactory results.

The blood freshly drawn consists of a fluid (the plasma) in which there are myriads of exceedingly minute irregularly spherical bodies (the corpuscles) swimming about. The *plasma* consists of two bodies, one of which, the fibrine, spontaneously separates from the other, the serum. The *corpuscles* are little sacs of delicate animal membrane enclosing a fluid. This fluid

has an acid reaction and its ash contains a monobasic alkaline phosphate. The fibrine of the plasma contains a monobasic phosphate of lime, though the plasma as a whole has an alkaline reaction, and its ash contains a great excess (11 per cent.) of chloride of sodium (common salt). The moist corpuscles constitute about one-half of the blood.

The relations of the fluid within the corpuscles to the plasma which surrounds them, is the same as that of the yolk to the white of the egg, and the ratio of the fixed base to phosphoric acid is alike in both cases.

1 : 1 in blood corpuscles, | 1 : 1 in egg yellow.*

The ash of albumen contains nearly one-third of its phosphoric acid, as monobasic phosphate (3.79 : 1.15) according to Poleck, while the same analyst gives for the analysis of the yellow 66.70 per cent. of phosphoric acid, of which 41.33 per cent. is free.

Under pressure the fluid contents of the corpuscles pass through their membranous walls, and through the walls of the nutritive capillaries. Such pressure exists when the blood vessels of a particular organ are engorged, as the blood vessels of the stomach always are in healthy digestion. Engorgement is the equivalent of obstruction. Under the pressure that follows, the fluid contents of the corpuscles will pass through their membranous walls and mingling with the relatively lessened plasma, pass on through the walls of the capillaries. It is obvious that the acid character of this mixed fluid is simply a question of the relative amount of acid fluid pressed from the corpuscles, and of alkaline plasma which the expressed fluid finds. Under such pressure the areolar tissue under the mucous membrane is charged with the mixed fluids of the corpuscles and plasma. This mixture contains, therefore, acid phosphates and chloride of sodium.

The mucous membrane of the stomach presents on its under surface the mouths of numerous microscopic tubes, which like a stocking are sometimes single blind sacs, or like a glove terminate in several blind sacks, like the glove fingers. In the bottoms of the tubes and along their sides are several closed spherical sacs, containing other lesser sacs and fluid within.

*Liebig's Jahr's Bericht. 1850, p. 559.

The tubes as a whole dip down into the spongy tissue that underlies the mucous membrane, where they are surrounded by the fluid poured from the surrounding net-work of nutritive capillaries, which fluid contains acid phosphates and chlorides.

Now by pressure and osmosis a portion of this fluid will pass through the walls of the gastric tubes, and the question is whether the fluid that goes through will contain free hydrochloric acid.

The experiments I have made are conclusive on this point.

By employing acid, phosphate of lime and common salt, I had this advantage, that as increased acidity on the one hand is a just inference from increased alkalinity on the other, and as increased alkalinity would be shown by the precipitation of phosphate of lime — a visible white powder — I could determine the qualitative fact without the difficulties and delay attendant on accurate quantitative analysis of the solutions on both sides of the membrane before and after the experiment.

With acid phosphates of lime in my earlier experiments I was embarrassed with the presence of sulphate of lime in the powder, so that what was at first supposed to be pure phosphate of lime, was found to be in part sulphate of lime. This sulphate was due to imperfectly washed parchment paper employed as a dialyser. This difficulty overcome the experiments were made with parchment paper prepared from German and Swedish filter paper, as well as with gold-beater's skin.

I employed acid phosphate of lime, successively, with chloride of sodium; chloride of potassium; chloride of ammonium; chloride of magnesium; chloride of calcium; with all of which there was the same kind of evidence of increased alkalinity on one side, and of course corresponding increased relative acidity on the other. The same effect took place with acid phosphate of soda and chloride of calcium.

It follows then from what we know of the composition of the blood, its condition in the walls of the stomach, and the structure of the gastric tubules that free or uncombined hydrochloric acid must find its way into the sacs at the bottom of the tubules. It is of course mixed with acid phosphates and alkaline chlorides. The sacs at the bottom of the tubules, by a secondary dialysis, concentrate the acid solution. Swelling

by endosmosis, and corroded by the acid juice, at length they burst, and the liquid contents, together with the disintegrated and partially digested membrane of the sacs, pass out to the stomach to constitute the gastric juice, the free hydrochloric acid and the disintegrated tissue (the pepsine?), to act in the liquefaction of the food.

---

17. On the action of Light upon the Bromide and Iodide of Silver. By M. Carey Lea, of Philadelphia, Pa.

The object of the following paper will be to contribute the result of some new experiments which, when taken together with observations already made, may add something to our knowledge of this very difficult subject.

It is an already ascertained and accepted fact that when bromide of silver is exposed to light, it undergoes decomposition with elimination of either bromine, or a bromine-compound, whilst it is itself reduced to a sub-bromide. This reduction is accompanied with a very distinct darkening. I have myself recently repeated this experiment under conditions of which I sometime back laid down as the basis for accurate investigation in this direction, viz., that thin films of the silver haloid, whether bromide or iodide, should be formed to the absolute exclusion of all organic matter. This I effected by silvering a plate of coarsely ground glass with specular metallic silver. Its film was then converted into bromide or iodide as might be desired, by plunging, in the one case into a bath of strong bromine water, in the other, into a solution of iodine in iodide of potassium. These plates when thoroughly washed, afforded films of the pure silver compound in a condition as convenient for experiment as if the compound had been supported on paper, collodion or other basis, and free from the fatal objection of the presence of organic matter, an objection which has already vitiated so many laborious examinations in this field. In the present case, it was found that such a film of bromide of silver by long exposure under a design cut out of an opaque object, gave a strongly marked image. The exposure required is a long one:—fifteen minutes of bright sunshine yielded no

result, but four hours gave a distinct impression — this is when the bromide is pure and isolated.

It is also an established fact that no such result can be obtained with *pure* iodide of silver likewise isolated. The most careful investigators, amongst whom I may cite Dr. Vogel of Berlin, have failed to detect any elimination of iodine, nor is a visible image obtained by an exposure of equal length to the foregoing.

These premises, correct in themselves, have led to certain erroneous conclusions, as I shall endeavor presently to show.

It is a well established fact that an exceedingly brief exposure to light, will, under favorable circumstances, produce a *latent image* on either iodide or bromide of silver; this image, invisible at first, is brought out by the application of suitable developers.

Now if we admit that iodide of silver is incapable of chemical decomposition by the sun's rays, we are forced at once to the conclusion that the latent image impressed upon it by light, is purely physical in its nature, since there is obviously no other alternative. This question is one which I have devoted much study to, and recorded results elsewhere strongly supporting the physical theory. It is not my intention to discuss that part of the subject here, but to examine the state of the question, more especially as regards the bromide of silver.

As bromide of silver is well known to undergo a distinct chemical decomposition by the action of light, it has never seemed necessary to apply to it the hypothesis of the formation of a physical image. It seemed probably more natural to suppose that, as a prolonged exposure to light produced a visible image, so a very brief one produced an image, which, though invisible, was of precisely the same nature as the visible image; that is, an image which though invisible from its excessive thinness and delicacy, yet formed the nucleus which by development became the visible image. This view was thoroughly consistent with the common analogies presented by photographic operations, in which, by the continued action of developers, an image though exceedingly thin at first, was gradually brought out to any desired strength. It will be my object here to show that this view, plausible as it is, is wholly

incorrect, and that bromide of silver under suitable conditions is capable of forming a latent image in which chemical decomposition plays no part, and which, therefore, must be considered as a mere molecular alteration, a physical, as contradistinguished from a chemical image.

But this physical image on bromide of silver is extremely and remarkably different from that found upon iodide of silver. The following statements will place the difference in a strong light.

1. The physical image is formed on iodide of silver isolated from all other bodies.

2. That on bromide is found, so far as my experiments have gone, *only in the presence of organic matter.*

3. The physical image on iodide of silver can be called forth in the presence of silver or of some other metallic body* only.

4. The physical image on bromide of silver can be developed in the complete absence of any metallic body.

These four distinctive positions are based upon a very careful series of experiments made by myself. The third of these positions, viz., that the invisible image upon pure iodide of silver isolated from all other bodies can be evoked in the presence of nitrate of silver only, has, I believe, never before been made,† and it is contrary to views which I have myself before entertained and expressed, which I avail myself of this opportunity to correct.

I proceed to the proof that a true physical image, apart from chemical decomposition, may be formed upon bromide of silver. This, as I have already said, takes place in the presence of an organic body.

Let a collodion containing bromide of silver and free nitrate of silver be extended on glass. Let this after setting be washed under a stream of water, and then be plunged into a solution of tannin, and dried. We have here a sensitive dry plate. Let this be exposed for a brief time in the camera. A latent image is formed. Let us now plunge the plate into a solution of pyrogallic acid; the image appears.

*I have shown elsewhere that these images may be developed with proto-nitrate of mercury.

†Until the experiments were made upon which this paper is based, it had never been ascertained whether the alkaline development in the absence of silver, so effectual with bromide of silver, was possible upon pure iodide. These experiments decided the question in the negative.

In the ordinary development of a dry plate, the above may or may not, form a part of the whole operation. Let us here separate it from any other operations and consider it apart.

What can have caused the latent image to appear? It could not have been that an infinitesimal *chemical* image of sub-bromide of silver acting as a nucleus, was brought up by the action of the developer to a visible intensity, because pyrogallic acid by itself has no power to do this, free nitrate of silver must be present, and in this case, none was so, as it had been removed by washing.

Thus, as we are absolutely excluded from this explanation, we find but one alternative, namely, that that portion of the film upon which the light had acted, was so modified thereby, that it was brought into a condition to be more easily decomposed by pyrogallic acid than the portion which had not been acted upon. We have just seen that we are excluded from the supposition that the modification was chemical; it must therefore have been molecular or physical.

Perhaps this may be rendered clearer by the following argument.

Bromide of silver is almost colorless; sub-bromide is deep blackish brown in color.

At the beginning of the operation, namely, before the exposure, the film was colorless.

At the end of it, when the image has been evoked, that image consists of deeply colored sub-bromide.

Now the coloration takes place only after the application of the pyrogallic acid, and it is evident that the *chemical* decomposition is simultaneous with the coloration; consequently the modification which took place during the exposure was not chemical, but purely physical.

It is unquestionably true that a prolonged exposure will produce a visible image independently of development; not an exposure of the length previously spoken of, measured by hours of bright light, for there the bromide was isolated, and here it is supposed to be in contact with more than one organic substance, but a comparatively short one in the camera may produce traces of an image. But the complication here is only a seeming one.

If it were necessary in the case of a bromide film (as it is in that of an iodide) that free nitrate of silver should be present, it then might be alleged that the strong visible image had been built up upon a thin invisible one. But this is not at all so. Admit in the case of a bromide film that a visible image *is* present, and let us argue on such a case.

The production of a visible image unquestionably indicates (so far as that image is concerned) chemical decomposition. But when we apply pyrogallic acid, we do not build up on that image, for silver in solution must be present for that. There being none present, there is no possible way in which the strong visible image can come out, except by the decomposition of portions of bromide of silver, not previously decomposed, and this by the agency of the pyrogallic acid.

If then this be so, if portions of the bromide film not decomposed by light, but simply acted upon by it, are subsequently decomposed by the action of pyrogallic acid, whilst those portions of the film not influenced by light, are not decomposed by the pyrogallic acid, then it follows that the action of the light was so far simply physical.

On such a plate, then, when exposed to light, two actions manifest themselves. There is a slow chemical action, which if it be continued sufficiently long, may be carried to any extent. But there is also a much more rapid molecular change, so that before the chemical image is visible, there has been formed a physical image capable of development by pyrogallic acid. And after a visible chemical image has been produced by prolonged exposure, there is simultaneously present a physical image which is capable of development by agents which have no action whatever upon the chemical image also present, and can neither add to nor take from the latter's intensity, but which bring out the previously physical image into a strength far greater than that of the previously visible image.

In the foregoing explanation I have carefully placed aside the *alkaline* method of development, because if I had argued upon it, it might have been alleged that the carbonate of ammonium employed (for example) dissolved bromide of

silver from the film and so afforded a soluble silver salt towards building up a visible image upon an invisible *chemical* nucleus. To avoid this objection I have selected a case in which it cannot be alleged that any trace of a soluble silver salt is present. We are thus thrown back, for the production of the developed image, upon the bromide of silver in the film, and thus we distinctly recognize the fact that the bromide of silver has, by the influence of the light acquired a property which it had not before, namely, a greater facility for decomposition by pyrogallic acid. As the act of this acquisition was attended by no chemical decomposition (which set in only when the pyrogallic acid was applied) it must, I think, be admitted that I have demonstrated that bromide of silver may, under favorable circumstances, receive a physical or purely molecular impression from light, leading to, but in itself distinct from, chemical alteration.

---

18. THE APPLICATION OF CARBONIC ACID GAS IN THE EXTINGUISHMENT OF FIRE. By E. L. Buttrick, of Chicago.

THE powerful effect of carbonic acid gas in checking combustion has long been known. Since the time of Sir Humphrey Davy, who was one of the first to describe its properties and portray its effects as a fire subduing agency, a vast amount of scientific research and mechanical ingenuity have been expended, in order to render it available in the service of man, to stay the progress of that destructive element, which at times defying all human control and all the simpler antagonisms of nature, destroys the proudest monuments of art, and sweeps away in an hour the accumulated results of centuries of toil.

During the past sixty years, both in England and on the continent of Europe, there have been numerous contrivances, many of them quite ingenious in their mechanical details, to accomplish that result, which when arrived at, it was known from the chemical properties of the agency employed, would give to man a more complete and immediate control of the destructive element of fire, and render it quite impossible for any extensive conflagrations to occur in the future, like

those which in the past have swept away the substance of thousands of people, and blighted the prosperity of towns and cities.

But until recently scientific research and mechanical skill have failed in all their endeavors to utilize the gas for this purpose to any considerable extent. Indeed, so elaborate had been the plans, so numerous the contrivances, and so frequent and complete the failures, that the author of a recent able and comprehensive English treatise upon the subject of extinguishing fires, expresses the opinion that it is useless to push such investigations farther, as in the very nature of things it is not possible by any mechanical contrivance, to render the use of carbonic acid gas at all available as an adjunct of the fire department, and a co-worker with man in protecting life and property from the perils of fire.

How ill grounded were the conclusions of the author referred to, the progress of events during the last few months has effectually demonstrated, for in various towns and cities of the United States, from Portland in Maine, to Milwaukie in Wisconsin, the fact has been demonstrated to thousands of astonished and admiring spectators; and as we are informed without a word of cavil, either from men of science or those unlearned in its mysteries, that the machinery was really invented; cheap, simple and powerful, capable of producing carbonic acid gas instantaneously and constantly, to an unlimited extent, which when so produced could be directed upon any part of a burning building by the same means, with the same precision, and with twenty times the effect that a stream of water could be directed from such engines as are now in common use.

Thus it seems to have been reserved for our own country to demonstrate that the inventive genius of her citizens which achieved for her such renown in the telegraph, the reaper and the sewing machine, was still unexhausted, and capable of winning new triumphs even where other nationalities had totally failed.

Before describing the invention to which it is the purpose of this paper to call your attention, it may be proper to give a brief sketch of other inventions for this same object, which

take precedence in point of time, and have attracted considerable attention in both hemispheres.

The first is a claim for an invention for utilizing carbonic acid gas in the extinguishment of fire by A. Vignon, of London, who procured a patent in the United States in 1862. His claim consists of the employment of a solution of carbonic acid gas in water, either at high or low pressure, for extinguishing fires on land or on board vessels, and the construction and employment of apparatus either portable or fixed, for extinguishing fires in which a solution of carbonic acid gas in water is prepared and stored up, and whence such solution is ejected with the requisite force without the aid of pumps. But Vignon describes in his claim no apparatus by which a solution of carbonic acid gas in water may be made available, and there is no evidence that any apparatus constructed by him for that purpose ever came into practical use. After this an attempt was made to apply free carbonic acid gas to the extinguishment of fire, by means of what is known as Phillip's Annihilator, but the invention proved impracticable, from the fact that the gas could not be directed with certainty upon any given point. The next attempt to utilize this gas for the purpose mentioned was more successful. It is an English invention, and is used to a very great extent in Europe, where it is known as Seavey's Fire Extinguisher, and in this country as Barogwanath and Van Wiskers' Improved Fire Annihilator or Portable Fire Extinguisher. It consists of a cylinder, in which by an ingenious contrivance the bi-carbonate of soda and acids are confined in solution, by which carbonate acid gas is generated in large quantities. To the cylinder is attached a faucet connected with a short hose. When the faucet is opened the pressure of the gas is such that the contents of the cylinder are thrown out with considerable force. These cylinders hold from three to eight gallons, and are designed to be carried on the back by persons using them at a fire. This little invention is of great value in extinguishing incipient fires, and is very extensively used in manufactories and large public buildings. It is entirely practical in its results, and so far as its power extends accomplishes the object for which it is designed. But something more was needed than the construction of a

simple machine for the production of a carbonic acid gas to quench fire in the incipient stages of combustion; and possessing more power than could be derived from the expansive force of the gas generated by the acidulous and alkaline solutions, and that want has at last been supplied. The apparatus consists of an engine in the ordinary form of a hand fire engine, except that the "tub" forming the body of the engine is divided into two compartments, in each of which is a force-pump of the usual form of construction, connected by proper metallic pipes and valves with a metallic air chamber firmly attached to the platform upon which the tub rests on a level with its bottom, and located between the two compartments. To the air chamber a hose is attached for the conveyance of the stream to any point desired, as in the case of an ordinary fire engine. In case of fire the separate compartments are supplied with water, and into each of them is thrown such acid and alkaline preparation as will best subserve the purpose of generating carbonic acid gas, having always in view economy of material in comparison with the results to be attained. The acid and alkaline solutions are inert or quiescent until they are brought together in the air chamber, and this is done by means of the pumps, which, worked in the usual manner, force at every stroke the contents of each compartment together. The effervescence is then instantaneous and complete, and the gas is evolved with such rapidity as to produce a pressure upon the air chamber equal to sixty pounds to the square inch, which can be maintained continuously, so long as the pumps are worked and the tanks supplied with water and the necessary chemicals. To the expansive force of the gas produced in the close air chamber, is added the additional arbitrary power of the pumps, whatever it may be, dependent upon the relative size of the cylinders to the power exerted at the brakes. It has been demonstrated by repeated experiments, that a small hand engine not larger than those used for garden purposes, and worked by two men, can throw a stream of carbonic acid gas and water combined, through a three-quarter inch hose and quarter-inch aperture at the pipe, that is more effective in subduing a conflagration than a stream of simple water thrown by the ordinary hand fire engine, when worked by the united power of forty or fifty men.

It is quite unnecessary for me, in such a presence, to detail the manifest advantages of this simple mechanical contrivance, whereby a well known scientific principle, the absolute certainty of which has long been known and demonstrated, is thus made applicable to useful ends, and rendered subservient to the welfare of man in the protection and preservation of property and life. Your own studies have led you into such investigations as will enable you to appreciate, far better than any suggestions of mine can aid you to comprehend, the vast and beneficent results to our country and mankind which are to flow from the increased power it gives to control that fiery element, which when under proper restraint is always the friend and servant of mankind, but once his master proves the enemy and destroyer of his property and his hopes.

---

19. On the Combining Power of Chemical Elements. By Samuel D. Tillman, of New York.

Since the promulgation of the doctrine of types and substitutions, the saturating powers of chemical bodies have been quite satisfactorily represented by symbols. The prominence which the subject of atomicity has recently assumed is partly the result of the plain and simple manner of illustrating it. By means of the new notation and several additional characters or signs, an ideal structure of a chemical body is represented; for of the real structure nothing is known. In it the saturating power of an atom or a combination of atoms is measured by degrees, the lowest power being taken as the unit of measurement. A given number of combining units require a like number of other combining units to complete the structure. The new body is always the result of duality, and its form or type will be retained while its chemical functions may be entirely changed by displacement; for certain parts of the structure may be removed provided a substitution of the same equivalence is made to preserve the molecular equilibrium. Thus the chemist assigns substitution-values to every simple and complex radical, and designating them by numbers, from one up to six, is prepared to combine his symbols in a process

of matching, which, although quite as simple as that with dominoes, affords the highest satisfaction, because it is always associated with the order, rapidity and precision of molecular changes. Yet this kind of chemical reckoning has one serious drawback, namely, the atomicity of the same element is not an invariable value. It is often decreased by a duplication of similar atoms, and in some remarkable instances among monads, it seems to be increased beyond its normal energy by a kind of induction which has not yet been accounted for. Saturating power depends on causes and conditions still wrapt in obscurity. Equivalence involves higher questions than those of quantity and quality, as these terms are applied to ponderable matter, for two elements having diverse chemical functions and widely-differing atomic weights, like hydrogen and chlorine, sometimes assume the same relative position in the mazes of chemical combination. In many instances attractive force or affinity seems subservient to fitness of place; hence has arisen the distinction of chemical and mechanical or molecular types, which can only imply that one kind of force is more effective than another in completing what may be termed the symmetry of the chemical structure. The smallest quantity of an element which can enter into combination has an invariable weight, and doubtless a normal sphere of attraction, which may be modified in its relations to that of another element, on changing the rate and range of the undulations of the interatomic medium through the agency of light or heat; or on disturbing its general equilibrium by what is termed electric force. It is evident that the minute changes which a molecule undergoes in chemical action involves a problem of motions too complex for complete solution.

Considerable diversity of opinion still exists among advanced chemists both as to the best form of types and the highest saturating power of several elements. Indeed the value of the same element varies so much under different conditions, that some writers, regarded as authorities, are now inclined to include all simple bodies in two classes, one embracing combining units expressed by even numbers, and the other those expressed by odd numbers. Some of the late conclusions of the ablest advocates of the new views would

seem to obliterate even this single dividing line of distinction. Professor Wurtz of the College of France, in his admirable lectures on "Types and Atomicity," is reported by the London *Chemical News* as saying: "Nitrogen seems to us diatomic in the binoxide of nitrogen." This body according to the new notation is a combination of one atom of nitrogen (14) with one of oxygen (16). The statement taken literally would place nitrogen in both classes alluded to. With equal consistency it may be asserted that nitrogen in the peroxide of nitrogen is tetratomic; and by a parity of reasoning nitrogen becomes a Proteus among types! Now, while it is admitted that only two-thirds of the normal saturating power of an atom of nitrogen is expended in combining with one of oxygen, it may with great plausibility be assumed that the remaining unexpended combining unit actually determines the value of this body as a monad radical. It cannot exist in a separate state, but two such compound atoms or atomoids will unite like two atoms of nitrogen, to form a molecule. In this manner the peroxide of nitrogen is shown to be also monatomic, because two atoms of the dyad oxygen have a substitution value of four, and exceed by one that of nitrogen. In one case the value of the radical is expressed by $3-2$, and in the other by $4-3$. Nitrous anhydride and nitric anhydride are formed on the water-type by the addition of an atom of oxygen to two of these radicals respectively. The lowest oxide of nitrogen, laughing gas, is an inert body having neither alkaline nor acid characteristics. Its atomoid contains two atoms of nitrogen and one of oxygen, and its combining power is expressed by $2 \times 3 - 2$. Should it hereafter be found to act as a radical it is here predicted that its value will be tetratomic.

An example bearing still more directly on the question of the atomicity of nitrogen is that of cyanogen, an atomoid of which contains an atom of carbon (12) and an atom of nitrogen (14). If these two atoms meet on equal terms, either the carbon atom becomes a triad, or the nitrogen atom a tetrad. On the other hand if it is assumed that both elements retain their normal saturating power, carbon as a tetrad and nitrogen as a triad we satisfactorily account for its monatomic substitution-value which is expressed by $4-3$. In its general

behavior this radical resembles the halogens; thus presenting an apparent anomaly in the formation of a compound having powerful electro-negative characteristics found in neither of its constituents, and proving that the predominence of atomicity in an element does not necessarily impart to the body of which it forms a part any of its chemical functions; in other words, that the atom of lowest combining value is not merged in that of the highest, but that, as between the two, the merging is mutual; the result being an absolutely new body bearing no resemblance to its constituents in a separate state, and exhibiting only that relation to them which arises from the law of differential atomicity. This law was expressed in a paper read by me before this Association in 1866, as follows: "The atomicity of a torso (that is an imperfect body having no separate existence) or of a radical containing one atom of an element united to one or more atoms of another element is equal to the difference between the normal saturating power of its components."

It is difficult to trace all the steps of the resulting atomicity in bodies containing only two elements when the atoms of both elements are duplicated, because such duplication involves an unequal decrease in their normal atomicity. However, in a homologous hydrocarbon series, in which the number of atoms of each element forming a molecule increase by arithmetical progression, the lowest supposable combination will be the measure of the atomicity of the sum of the carbon atoms in every member of the series; for example, the body not yet isolated consisting of one atom of carbon and two atoms of hydrogen would have a combining power expressed by 4—2; accordingly we find that ethylene, and every other homologue has the value of a dyad and may be united with two atoms more of hydrogen. Thus in the hydride of methyl series, the saturating power of a given number of condensed carbon atoms is expressed by the number of hydrogen atoms in the combination, which is the measure of the power of the same number of carbon atoms in other bodies; for example, the composition of hydride of ethyl proves that two atoms of carbon play the part of a hexad, therefore the saturating power of acetylene, composed of two atoms of carbon and two of hydrogen is

expressed by 6—2. This branch of the subject cannot be presented even in outline without the use of symbols; and as the symbols I use are not yet generally adopted, and might divert attention from the principal points to be elucidated, I do not propose to introduce them in the present paper.

Our subject in its broadest meaning includes investigations relating to the diversity of composition in chemical bodies. The cause of this diversity may be assigned in a general way to affinity, by affirming that a powerful electro-positive element combines most directly and intimately with a powerful electro-negative, producing at once a compound in the most condensed state; as for example, sodium and chlorine in forming common salt, while elements having less affinity may unite by degrees in a variety of forms, as seen in the combinations of oxygen with nitrogen, sulphur and the halogens. This tendency to diversity is more remarkable among metalloids than among metals, for the latter, with one exception are solids, and in general, are not readily brought into the liquid and gaseous states.

Nothing in the whole range of nature's operations excites greater wonder than the fact that a large majority of the chemical bodies derived from the organic kingdom are composed of the same three or four elements, which by diversity of combination give as great a variety of characteristics. In ammonia, nitric acid, laughing gas, olefiant gas, ether, alcohol, aniline, water, spermaceti and the gaseous mixture forming the atmosphere, a combination of two or more of these elements exhibit the most striking contrasts. On examining hydrogen, oxygen, nitrogen and carbon, separately, among other peculiarities will be found the following:

1. Three of them are permanent gases which have resisted all attempts to liquify them by means of cold and pressure; and the fourth is a permanent solid, which in its purest form, the diamond, has maintained the same state under the highest degree of heat yet applied.

2. These elements are colorless under all attempts to change their natural state, yet by combination they evince every tint and shade of color.

3. Taking the weight of the hydrogen atom at 1, the combin-

ing numbers of oxygen, carbon and nitrogen are low multiples of the prime numbers 2, 3 and 7, respectively; from which the inference is drawn that harmonic relations exist between the vibrations or undulations produced by or in these elements. These coincidences may seem to have little significance, but when taken in connection with the remarkable relations of atomic weights of three elements in several different groups discovered by Dumas, and the general difference in the atomic weights of nearly all the elements, as pointed out by M. Carey Lea, they indicate the connecting links between form and motion, an allusion to which will be found in a previous paper on harmonic relations, and seem to confirm the surmise that all chemical elements are the offspring of the universal æther, first quickened by the divine Author of all, and ever moving in accordance with His stupendous plan.

To return to nitrogen; its connection with the highest prime number which belongs to harmony, would indicate that it does not frequently and readily enter into chemical combination. A natural inquiry arises here, whether any element supplies the missing prime number? The atomic weight of calcium is a multiple of the prime 5. This brilliant yellow metal enters into the composition of all bone, and, in combination with oxygen and phosphorus, forms the foundation of the animal structure. Ample reasons may be adduced for not finding in the atomic weight of phosphorus more simple numerical relations; however some significance must be given to the fact that phosphorus forms just one-fifth of the weight of animal phosphate. Two other elements entering into the animal organization are connected by similar numerical relations with those already mentioned, sulphur with oxygen, and iron with nitrogen. Other interesting coincidences might be pointed out did they not give too much prominence to views which at present will be regarded as merely speculative.

4. The normal saturating power of hydrogen, oxygen, nitrogen and carbon, are respectively as 1, 2, 3 and 4. Binary combinations of these elements will indicate the direction in which we are to look for the principal cause of diversity in the composition of organic bodies. Of hydrogen and oxygen only two combinations are known: of nitrogen and oxygen,

five; of hydrogen and nitrogen, one; of carbon and nitrogen, one; while of carbon and hydrogen over thirty have been thus far isolated. The remarkable tendency of the lightest gas and the most impenetrable solid to combine in definite proportions, and to condense by degrees, thus forming homologous gases of different densities, as well as liquids and solids of different boiling points, must be assigned as the principal cause of the diversity of composition found in the many hundred compounds of which these two elements form a part.

Berthelot has recently demonstrated, by numerous experiments, that alcohols, ethers, aldehydes, alkalies, acids and other compounds containing Carbon, may be decomposed by the action of hydriodic acid and heat, and converted generally into homologues of the hydride of methyl series; and he has expressed the opinion that the carbon in all organic bodies may be thus saturated with hydrogen. The startling conclusion to be drawn from his experiments, the facts set forth in this paper, and all previous experiments showing the disposition of hydro-carbons to combine with other elements is, that the great antipodes in extension, carbon and hydrogen, play the principal parts in the composition and decomposition of all bodies derived from the organic kingdom.

---

20. On Some New Chemical Relations of Metallic Aluminum. By Professor Henry Wurtz, of New York.

The more important of the facts which led my investigations into this direction were observed in December, 1864; and some account of them was first offered at a meeting of the New York Lyceum of Natural History in November last.

To render myself fully intelligible at this time (especially as no publication has been made by the New York Lyceum, or otherwise, as yet) it is necessary that I resume concisely the whole subject from the beginning.

Aluminum, the most abundant of metals, has been deemed a chemical paradox. Though among metals one of the longest to elude isolation, and not known to exist in the native state, yet it is found to assimilate itself, when pure, with gold, plati-

num, etc., a class of metals always found native, and which varies diametrically from aluminum in their relations to electro-negative elements. My observations, as I believe, furnish the key to unlock this paradox.

In a paper in the "American Journal of Science," March, 1866, I have shown that aluminum presents a parallelism to most *native* gold in its relations to quicksilver; both showing normally a powerful superficial repulsion for the liquid metal, while in both cases the addition of a trace of sodium converts the repulsion into a positive and powerful attraction.

The analogy holds good also in another point, namely, that mere abrasion or incision of either metal, thus developing an artificial surface, shows immediate adhesion of the quicksilver to such new surface. Here, however, the analogy ceases, and in fact this adhesion is far more difficult to exhibit with Al than with Au, the former metal presenting the singular phenomenon of an extension of the lamina, which is passive or repellent to quicksilver, to some appreciable depth under the surface; while in the case of Au this lamina is but the thinnest possible film. Hence it is, probably, that the remarkable relations of Al to Hg escaped observation until my experiments with amalgam of sodium. Also as I have remarked in the paper before cited, while Au is quickly penetrated by Hg at the points of contact, forming an amalgam; Hg after being made to adhere to Al, either by rubbing, or by Na, is readily "wiped off again clean, as water may be from glass." In this latter point the analogy of Al is stronger with platinum than with gold.

Both cast and wrought aluminum behave alike in these respects. If a piece of sheet aluminum, with edges freshly cut or broken, be instantly immersed under quicksilver, penetration of the latter at such edges generally commences at once, and proceeds rapidly; so that in a brief time the sheet *may be split* (either partially or wholly) into two, brightly enfilmed on their inner surfaces. The phenomenon is still more strikingly shown by bending sharply under quicksilver a piece of tolerably thick sheet aluminum. Sometimes at the first bend the metal, otherwise so tough, breaks short off, generally with a strongly marked laminated fracture, the two exterior layers

parting along separate lines, so that brilliantly enfilmed internal surfaces are exposed.

If such enfilmed surfaces be now exposed to the air, best after wiping off the excess of quicksilver adherent, a singular phenomenon will be witnessed. Their brightness is instantly tarnished, and in a few seconds *an efflorescent growth appears*, so rapid as to be visible to the eye; this efflorescence being composed, as I have found, of pure hydrate of alumina, in the form of single threads and filamentary bunches. Under the magnifier the growth of the feathery mass of slender curved filaments on such a surface of aluminum, is a sight of a very peculiar and unprecedented character. The more moisture there is in the air the more rapid the action. The evolution of heat accompanying the combustion is readily perceptible to the hand; and, by certain modifications (sodium being used to effect a rapid enfilming) may easily be made intolerable thereto!

Two new and startling conclusions are thus forced upon us, which I shall put into the form of axioms or postulates of a new theory of the nature of aluminum, to wit:

1. Our ordinary or normal and stable form of aluminum is in reality correspondent to what is known as the "passive" (usually highly *unstable*) form of other metals (as the well-known "passive iron" for example); or, as I should prefer to say:—the resultant of the forces that aluminum encounters when isolated, under ordinary conditions, places its surface in an *electronegative* state or *attitude* towards oxydizers.

2. Absolute *molecular* or *chemical* contact with quicksilver produces an extraordinary, unstable and abnormal form, corresponding to the normal forms of sodium and potassium; or, as I would express it:—quicksilver, by absolute contact, *induces* upon the surface of aluminum that intensely active *electropositive attitude* towards oxygen and water which we have hitherto known only as an attribute of the alkali-metals.

In my experimental investigation of this theory, the following questions and hypotheses have been examined:

Question 1. Most essential of all. May it not be due merely to a very fine division or diffusion, as in an amalgam of Al?

A negative reply is inferable from the fact that fine division.

otherwise produces no such phenomenon; but a crucial test is to attempt to produce, what on this hypothesis should be producible, a solution or fluid magma of Al in Hg presenting the phenomenon. Neither with hot nor cold Hg, with nor without Na, can any such solution or magma be obtained. In fact, even if the coating of Hg on the Al has an appreciable thickness, no action takes place, except at the edges of immersion.

The penetration I have described of Hg between the laminæ of a sheet of Al appears similar to that in Professor Henry's lead-syphon.

Question 2. Is the new phenomenon to be classed with those of the *pyrophori* (like iron reduced by H, Pb from the ignited tartrate, etc.) which burn spontaneously to anhydrous oxyds, independently of atmospheric moisture? Answered readily by enclosing enfilmed aluminum in jars of air previously dried by sulphuric acid and by chloride of calcium. It was found to remain bright for hours.

Question 3. Is the phenomenon then analogous to those of K and Na, when exposed to the air; and accompanied by the production of a hydrate? Ignition of the effloresced product in a glass tube, and collection of the condensed water, answered this question in the affirmative.

I infer from these facts that unless we have here come upon an entirely new class of relations, destitute of any known analogue, we must conclude that Al in this new state corresponds with the ordinary states of the alkali-metals. The possibility of passive forms of K and Na is here suggested; and that of a permanently passive state of iron, corresponding to the ordinary form of Al. This latter possibility is not without support on other grounds; as analogies with other metals, and particularly the many recorded cases of accidental production of iron highly resistant to oxygen; insomuch that most chemists find it necessary to explain its usually rapid rusting, by suppositions of *voltaic* actions arising from lack of homogeneity, and so on; thus implying a belief in a natural highly resistant, if not wholly passive form. I may add that the passive state if discoverable, will doubtless be found inherent only in a *pure* iron, hardly attainable by the modes of manufac-

ture in use; and suggest trials by modes analogous to that for Al, namely by decomposition by Na of ferrous or ferric chloride or other haloid iron compound in anhydrous form. Pertinent hereto is the consideration of the familiar modes of producing external passive films on commercial iron, involving voltaic conditions not at all understood.

Much time has been spent by me in testing the question of the existence of an amalgam of Al or of any real solvent action of Hg upon Al, but with negative results. Fine filings or scrapings of Al or *leaf* Al may be incorporated with Hg, by rubbing in a mortar, into a pasty mass. This mass, however, does not exhibit the phenomenon of spontaneous combustion, simply because of the *complete* envelopment of each particle of Al by the Hg; all access of atmospheric moisture to the former being thus debarred. An experiment, remarkably confirmatory of the opposite internal and external conditions of Al, and in other respects highly curious, is made by simply throwing a piece of Al leaf upon a clean pool of Hg. Instant and powerful adhesion is manifested, the leaf flattening itself out just as a leaf of Ag would under the circumstances. This appears, however, to be due merely to the *cohesive* attraction of the two films of air condensed upon the two metallic surfaces; for no spontaneous oxydation of the Al ensues, after ever so prolonged contact. If now the edges of the leaf be simply *torn*, while lying on the Hg, at every such torn edge real *chemical adhesion* is developed, and a conversion of the leaf into a white powder of hydrate of alumina will commence, which will propagate itself, though *with extreme slowness* across the whole. The slowness of the change in this case being clearly due to the fact that the action of the Hg is but through the excessively tenuous internal lamina of the leaf, which is not passive to Hg.

The inference might be made by some, that it will be necessary, to avoid presumed danger from spontaneous combustion, to keep Al and Hg carefully apart, at least when in any considerable masses; but, for several reasons (though such precaution will actually be necessary on other grounds) little danger may be apprehended on the ground of *fire:* 1st, because of the very high capacity of Al for heat; 2d, because of the

rapid development of an obstacle to the access of moist air, through the accumulation of the feathery hydrate; 3d, and most conclusive: through the quick dissipation of the exciting agent, the Hg, by elevation of heat. Enfilmed Al, when heated above ebullition of Hg, assumes a surface which, while dull and metallic, is again passive.

Still, contact of Hg will be destructive, to a ruinous extent, to all articles made of Al; a conclusion from my observations of obvious practical and commercial importance.

Enfilmed Al in water produces but a slow effervescence, and the question occurs whether the insolubility of the hydrate interferes. Addition of sulphuric acid, however, does not set up any more rapid action; so that it seems likely that the intensity of the action in air may be due to coöperation of the free oxygen of the air.

---

21. On the Condition of our Knowledge of the Processes in Luminous Hydrocarbon Flames. By Professor Eugene W. Hilgard, of Oxford, Mississippi.

For more than half a century after Sir Humphrey Davy's important investigations of the subject of flames, the experiments and explanations of that eminent philosopher have passed, unchallenged and almost unchanged, into text-books and lectures. As regards luminous flames especially, he established the necessity of the presence of a solid incandescent body to produce useful luminosity; and in reference to the flames of hydrocarbons in particular, he suggested that the liberation of carbon in them was owing to the combustion of the hydrogen of the compound in advance of its carbon, the latter being heated to incandescence by an oxyhydrogen flame, as it were, and failing to be consumed until all the hydrogen was first oxidized.

It is remarkable that an explanation so directly at variance with the daily experience of blacksmiths, and with a lecture experiment performed even in the most elementary course of chemistry, could so long have passed current; for the decomposition of steam by ignited charcoal into hydrogen and carbonic

oxide gases is an old observation. Nay, most of us who have performed this experiment on the lecture table, have been in the habit of passing the gas through lime water or potash lye, to free it from *carbonic acid* before showing its properties. And yet we were taught, and have ourselves taught, that in the flame hydrogen burnt first and carbon afterward, and any doubt on the subject was quieted by a reflection about the influence of "chemical mass" on affinity.

When, in 1852, I was a student in the laboratory at Heidelberg, to which Bunsen had then but just been called, he suggested to me as an interesting subject of investigation, the composition of the gases in the various portions of the flame; stating at the same time that the prevalent opinion concerning the respective affinities of hydrogen and carbon for oxygen were certainly erroneous: that when mixtures of carbonic oxide and hydrogen were exploded with oxygen in the eudiometer, the distribution of the latter took place according to some law dependent upon the relative proportions between the combustible gases, and also the amount of oxygen present, but discriminating widely in favor of carbon. The experiments upon which this opinion was based are, probably, those given in his "Gasometric Methods" at the conclusion of the chapter on the combustion of gases.

In order to study the processes taking place in the flame, it became necessary to investigate first of all the composition of the gases in the interior cone, from which the flame derives its substance. I constructed a lamp adapted to the introduction of a suction tube into the flame from below, and made a series of fifteen analyses of the gases so collected at various points of the interior cone of the flames of beef tallow, and of wax. The low temperature known to prevail in this portion of the flame rendered it least adapted to elucidating the mooted question of affinities; but being the generator from which the other portions of the flame are supplied, a knowledge of its component gases was indispensable. My analyses (published in Liebig's Annalen der Chemie und Pharmacie, vol. xcii, p. 129, 1854) showed, however, the existence even in the highest portion of the cone, of free hydrogen with a large excess of carbonic oxide and carbonic acid; the amount of hydrogen varying

but little from base to point, while the carbonic acid increased in about the same ratio as nitrogen, *i.e.*, in proportion to the oxygen entering the flame. Bunsen as well as myself failed, however, to draw the legitimate conclusions from these facts at the time; the more as, with the materials I used, it was impossible to follow the formation of water by progressive oxidation.

The latter difficulty was avoided by Landolt, who, two years later, took up the same subject, the failure of my health having rendered doubtful the prospect of my ever being able to resume it, so as to carry out the proposed investigation of the other parts of the flame.

Landolt* used illuminating gas of known composition, and was therefore enabled to determine the deficient factor in my analyses, viz., water. So far as comparable his results in general confirmed mine. He felt compelled by the increase of free hydrogen in the higher parts of the flame examined by him, to assume the occurrence of a reaction between free carbon and (pre-formed) water; but he also failed to draw the inevitable conclusion as to what *must* happen in the luminous cone.

A later research of Lunge† on the composition of the gas contained in the interior cone of the flame of a Bunsen's burner, must have led to the truth of the matter, by showing how little oxygen sufficed to render a flame non-luminous when previously mingled with the gas. I have not seen Lunge's memoir; but he likewise seems to have failed to draw the important conclusion of which his analyses must have contained the elements.

Next, in June, 1860, comes a memoir of Erdmann,‡ who in discussing the principles upon which his gas-tester (a modification of Bunsen's burner) is based, first distinctly enunciates that, according to his experiments, the carbon in a flame is oxidized *before* the hydrogen, and that the separation of carbon upon which luminosity depends, is due to heat alone, as would be the case were the gas passed through a red hot tube.

Finally, eighteen months later, we have an elaborate and

* Pogg. Ann., vol. xcix, p. 389.
† Annalen der Chemie und Pharmacie, vol. cxii, p. 205.
‡ J. pr. Chem., June 1860, p. 241.

elegant research by Kersten,* wherein he proves by eudiometric experiments that (at least within the limits of the proportions employed by him) whenever a hydrocarbon is exploded with oxygen insufficient to burn more than the carbon to carbonic oxide no hydrogen at all is oxidized; but that as between carbonic oxide and hydrogen, the formation of carbonic acid on one hand and of water on the other depend upon "chemical mass," as Bunsen had already shown.

This question has therefore been peremptorily settled by decisive experiments, as much as eight years since. Yet the latest editions of text-books published in this country, and even such as, like the excellent work of Messrs. Eliot and Storer, claim to represent the latest state of the science, still retail the old error regarding the succession of oxidation.

There is another point which, though I took special pains to demonstrate the facts fourteen years ago, is still incorrectly stated in almost all text-books as well as books of reference. I allude to the definition of the several essentially distinct parts of the flame. *Three* only are usually mentioned, viz., the inner cone, the luminous portion, and the outer, faintly luminous envelop or veil. Yet Berzelius already distinguished the very important fourth part, viz., the blue cup-shaped zone surrounding the base of the flame, which is as sharply defined from the inner cone, as from the outer veil with which it is usually confounded.

That this blue cup is identical with the blue cone of the blowpipe oxidation-flame, is stated by Plattner in the first edition of his work on the blowpipe. Strangely inconsistent with his own definition, he nevertheless teaches that the blue cup is formed by the combustion chiefly of carbonic oxide, produced by the "first and weakest effect of the heat on the fuel" — an assumption as little justified by experiments regarding such action, as was that of the combustion of hydrogen previous to carbon. It is palpably inadmissible in reference to the blue oxidation-cone, which is of course identical with the flame of a Bunsen's gas-burner — the supply of oxygen being, in both cases, sufficiently great, and so mingled with the entire gas, as to suppress the separation of carbon.

*J. pr. Chem., Dec. 1861, p. 290.

The part performed by the blue cup, viz., that of a self-heating retort with walls impervious to oxygen, in which dry distillation is accomplished; its theoretical import, as the counterpart of the luminous portion, where the same gases are burnt with evolution of light, render the neglect with which it has been treated doubly surprising. That it is totally distinct from the outer veil is readily perceived when the eye is protected from being dazzled by means of a screen of the shape and size of the luminous hollow cone. The veil is then seen surrounding the blue cup as well as the higher portions of the flame, and is thus proved to be nothing more than a zone of glowing gas; which of course, however, cannot be strictly defined from the luminous envelop, the oxidation being a gradually progressive one, from the highly luminous central portion to that brownish, semi-transparent zone of transition, where the carbonic oxide, burning simultaneously with hydrogen, fails to produce its characteristic blue tint because of the excessive temperature existing there. The same is the case when it is burnt by itself from a jet kept at a white heat.*

The inner cone, too, is still incorrectly defined as "the space containing the combustible vapors and gases generated from the wick." This would lead any one to suppose, that the external atmosphere had only the effect of burning off the outside of this gaseous mass, and some text-books have gone far in the graphic delineation of this process. My analyses first proved, fourteen years since, that even in a tallow flame, characterized by an excess of fuel over the available oxygen, the per centage of nitrogen gas does not fall below fifty-nine per cent. in the lowest part of the flame, and increases to seventy-six per cent. at the point of the inner cone, with from ten to fifteen per cent. of carbonic acid. The products and educts of combustion therefore greatly exceed the combustible gases, and essentially modify the processes thereafter occurring; for it is clear that in the luminous portion, the water and carbonic acid must in part at least again yield up their oxygen, before final combustion.

On the latter point, I have never been able to agree with Bunsen, who maintained that the combustion of the carbon in

* See Gmelin's Handbook, art. Carbonic Oxide.

the luminous envelop must be effected by free oxygen penetrating into it; because its combustion at the expense of the reduction of carbonic acid and water, could not be the cause of such intense production of light and heat; and upon his authority I reluctantly adhered to this view in my paper, above referred to. I believe with Kersten (l. c. p. 314), that the burning surface of the luminous cone, where carbonic oxide and free hydrogen meet the oxygen of the air, is the sole source of the heat which causes the intense ignition of the interior, and that the oxidation of the free carbon is effected only through the medium of the intensely heated carbonic acid and steam which penetrate from the outside. I habitually compare the process taking place in the luminous envelop, to what would happen if illuminating gas were passed through an ignited tube into which steam and carbonic acid are injected through lines of lateral orifices. The ignited carbon set free by heat would render the interior atmosphere intensely luminous at the near end, but the luminosity would rapidly decrease toward the far end, where we would have such a mixture of carbonic oxide and hydrogen as that which, I conceive, is burning on the surface of the luminous cone, and which *there*, above the point of the latter, produces the maximum temperature.

Unfortunately, a direct solution of the question by an investigation of the gases contained in the luminous envelop, is exceedingly difficult. Much remains to be done in the study of the details of the differentially variable processes by which the familiar phenomenon of a luminous flame is produced; but what *is* known is at least worthy of a place in the text-books.

---

22. THE METEORITES FROM POLAND AND MEXICO. By Dr. Lewis Feuchtwanger, of New York.

(ABSTRACT.)

THE specimen here exhibited fell 30th January, 1868, in Pultuosk, near Warsaw in Poland, and I saw a short notice of it in a public print, of the wonderful Bolis of Warsaw, stating that on a starlight night the citizens of Warsaw gazed petrified with fear at the rapid approach of an immense ball of fire,

which at last bursted over their heads with a noise and shock, such as never had been heard or felt before on the surface of the earth. After the globe bursted, each of the pieces in turn broke up, until parts of the mass, before reaching the earth, were broken up in small masses accompanied by discharges resembling those of artillery, and several of them resembling more nearly those of many regiments of small arms; and during these several discharges millions of pieces, mostly rounded masses with a black metallic coating, were found scattered in the neighborhood, which from appearance are the Lithosiderites according to Shepard's last classification. Mr. Daubree has presented to the French Academy 942 pieces of these broken bodies, and an analysis of their exact composition may be expected from that gentleman, but I have not seen any notice of it as yet.

The Mexican Meteorite. But a small piece could be chiselled off on account of its extreme toughness from a lump of 800 pounds weight; in all were eight pieces of the meteoric irons landed in this city August 1st, weighing in the aggregate about 4 000 pounds, from a bark sailing from Texas from where they were sent by Dr. H. B. Butcher, who procured these masses in the spring of 1868, but they were known to exist ever since 1837 by Dr. Long, who resided then in Santa Rosa, and who gave a description of the phenomena attending their appearance, of the excitement produced by many citizens who were also eye witnesses of that catastrophe; and that gentleman stated that he went in company with many friends in search of the locality where these meteorites descended, about ninety-five miles west of Santa Rosa, but was then unable to trace the spot.

Having but a small piece at command for analysis, I took repeatedly the specific gravity and found it to be 7.50 and a hardness of 3; its color is silver-white, the form of the masses is quite angular, but rounded at the ends, a number of air holes from one to two inches in extent is visible on all the eight masses, and from first appearance they resemble animal remains.

The chemical analysis yielded me ninety per cent. iron, nine per cent. nickel, and about one per cent. composed of other

foreign substances, with a trace of manganese and cobalt; by treating the small piece at my disposal with nitric acid, the Widmanstidi figures were very distinct. It was impossible for me to ascertain whether the Schreibersite, Troilite or Chamasite can be traced.

Having reflected lately on the subject, and comparing the remarks of Mr. Forrest Shepard in his letter to Professor Charles U. Shepard, of article forty-seven, page 347, in "American Journal of Science and Arts," 1866, of Major Hamilton's journey from Santa Rosa westerly to Naricmento about ninety miles north-west from Santa Rosa, who described the narrow valley of a quarter of a mile square partially overgrown by palmetto palms, and there he saw fourteen ponderous masses of native iron, the largest of which rose upwards of four feet above the ground, having the form of a bee-hive. These masses Professor Shepard designated as the Boranza Meteorite, which, according to his reported chemical composition, appears to correspond with the results obtained by me. Mr. S. states that the residue form the solution in nitric acid and was of a pulverulent nature, which was also the case in my examination, and I am forcibly impressed with the idea that the fourteen masses spoken of by Mr. Forrest Shepard, are identical with the eight masses which Dr. Butcher procured in Mexico.

It is likewise apparent to me that Major Emory's iron masses having been found ninety miles north-west from Santa Rosa, 28° latitude and 101° longitude, are identical with these eight masses. I am farthermore inclined to the belief that the meteorite in the Smithsonian Museum at Washington, weighing 250 pounds, and stated as having occurred but fifty-six to sixty miles from Santa Rosa, Coahuila, is one of the fourteen masses of iron spoken of by Major Hamilton.

There is no doubt but what the fall of those iron masses created great excitement in northern Mexico in the department of Coahuila about thirty years ago; the eight masses now in Philadelphia are unquestionably a part of the fourteen masses mentioned. The inhabitants of that neighborhood were all cognizant of the existence of all these masses. They all fell at one time and at one and the same locality.

23. THE STATICS OF THE FOUR TYPES OF MODERN CHEMISTRY, WITH ESPECIAL REGARD TO THE WATER TYPE. By Professor Gustavus Hinrichs, of Iowa State University.

IT has taken exactly half a century to discover and fully develop the fundamental law of chemical combinitions; and this law is a paradox, for it states that

$$1+1=2 \qquad 1+3=2$$
$$1+2=2 \qquad 1+4=2$$

and in general $m+n=2$, for the component and resultant volumes of elements and compounds in the gaseous condition. The memorable experiment of *Gay-Lussac* in 1805, establishing the combining ratio of oxygen and hydrogen to form water as one to two, opened this line of research; in A. W. Hoffman's admirable "Introduction to Modern Chemistry, 1865," the law is clearly stated, as above, and fully supported in all its bearings by ingenious and decisive experiments.

But what is the theoretical consequence of this remarkable law, and what is its cause? This question seems not yet to have been touched upon, except in my *Atomechanics*, June, 1867. We shall here consider this question independently of the constitution of the elements.

In older branches of physical science similar laws are known and explained. Thus one, two, or three or four equal weights may balance the same power, by means of a lever or a hydraulic press; but in this case the law has long ago been fully explained and therefore ceased to be a paradox.

In chemistry, the law is in general as follows: The volumes of two elements A and B in gaseous condition combine according to some of the following modes or *types:*

1 volume A + 1 volume B giving 2 volumes AB.
1 " A + 2 volumes B " 2 " AB_2
1 " A + 3 " B = 2 volumes AB_3
1 " A + 4 " B = 2 " AB_4

But besides these types there are many others, as

2 volumes A + 3 volumes B = 2 volumes A_2B_3

and indeed, organic compounds prove that in general

m volumes A + n volumes B give 2 volumes A_mB_n

The high theoretical interest of this law is very much increased yet by the fact that the elements have a definite predilection for some one of these types (*atomicity* of the elements). This is represented, together with my very convenient general symbols (*greek* initials) of the groups of elements, in the following table:

HINRICHS' CLASSIFICATION OF THE ELEMENTS.

Metalloids:	*Metals:*
MONATOMIC.	
Chloroids, X; Fl, Cl, Br, Io.	*Kaloids*, Κα; Li, Na, Ka, Rb.
Pantoids, Y; H.	
DIATOMIC:	
Sulphoids, Θ; O, S, Se, Te.	*Chalcoids*, Χα; Ca, Sr, Ba.
	Cadmoids, Κδ; Mg, Zn, Cd, Pb.
TRIATOMIC.	
Phosphoids, Φ; N, P, As, Sb, Bi.	*Ferroids*, Σι; Al, *Fe*, Rh, Ir.
	Sideroids, Σδ: Cr, Mn, Fe, Ni, Co.
TETRATOMIC.	
	Titanoids, Ττ; C, Si, Ti, Pd, Pt,

and several other groups. These are sufficient for our present purpose (*Atomechanik*, Abschnitt I). The symbols here introduced for natural groups of elements will be found exceeding convenient.

The law of combining volumes is entirely independent of the nature of the combining elements, and must accordingly be due to a simple *mechanical* cause. Thus both H_2S and O_2S are following the same law $2 + 1 = 2$ volumes, although in H_2S we have two atoms of a monatomic element, but in O_2S two atoms of a diatomic element. So also N_2O and O_2N follow precisely the same law; and yet in the one we have two atoms of nitrogen, but in the other two atoms of oxygen.

Hence it remains to find the mechanical cause of this condensation; but that can only be done by referring to the constitution of *gaseous* elements as now understood. It matters nothing, whether we take the molecular or the atomic view, that is whether we suppose the formula of Hydrogen to be H (1 vol.) or HH (2 vols.). We shall adopt the former view, because the latter appears to be disproved by *Tyndall's* experiments on the absorption of radiant heat. (See my paper in the *American Journal of Mining* for March 2, 1868.)

Let us then represent a number of atoms of hydrogen by an

equal number of adjacent and equal squares, as *Hofmann* does; thus the same number of atoms of any other truly gaseous element will be represented by the very same figure, so far as magnitude of the *space occupied* is concerned.

But it is very often forgotten that the ponderable atoms far from FILL these atomic volumes (squares representing *cubes*), and that the *degree of condensation* for the different uncombined elements is very different.

In regard to the first circumstance, it is well known from the excessive increase in volume upon the conversion of a liquid into gas, and by the high expansibility and compressibility of gases themselves, that the solid, ponderable particle — called the ATOBAR in Atomechanics — FILLS but a very small portion of the *atomic volume* occupied by this atom, that is inside of which there is no other atobar. Whether it be vibratory caloric motion (*Krönig*, *Clausius*, *Tyndall* and others) or whether the particles (atobars) be surrounded by an elastic, repellent medium (ether; see older and also present writers, *Tyndall*) this question need not concern us here. But it may be well to give one or two illustrations. A minute *dot* in the centre of the square representing the atomic volume will therefore indicate the active *atobar*. So also we say a person of about 5 cubic feet occupies a room, say 15 by 10, by 10 or 1500 cubic feet; hence only about $\frac{1}{300}$ of the actual space is *filled* by the person! The same room will hold a visiting friend or two, and be not actually *filled* yet. So inside the space of our earth's orbit only $\frac{1}{1000000}$ part of the globular space is filled by the sun, and yet the sun *occupies* and pervades by its light, heat and gravitating power, a much greater space, reaching beyond the stars!* So it also is in matter, at least in the gaseous condition. The various elementary atoms combined are as the physical double and multiple stars in the heavens.

In regard to the second point it may be enough to call to mind that one atomic volume of hydrogen weighs *one*, but the same atomic volume of iodine weighs 127, hence is 127 times as much filled as the hydrogen atomic volume. So also one

*Of the globe described by a radius equal to the distance to 61 *Cygni*, the solar system only *fills* $\frac{1}{2.10^{24}}$ part (2 followed by 24 zeros!).

atomic volume of oxygen contains 16 times as much of matter, as one atomic volume of hydrogen. From this we can readily see how one, two, three or more hydrogen atoms (their ATOBARS) may be brought into the same atomic volume occupied by but *one* atom of uncombined hydrogen.

Here it is important to notice the following direct consequence of condensation. If *two* atoms of hydrogen are condensed to one atomic volume, their two atobars form a straight line (two points always determine a straight line). If *three* hydrogen atoms are thus condensed, not only do these three atobars determine a plane and the three corners of a triangle, but they will form an *equilateral triangle* (of 60 degrees) because the three atobars being alike, their mutual action will be alike upon one another. So also if *four* are united to one group inside of *one* atomic volume, then four will form a *square*, unless some other force deviate them more or less from this position, just as for example the sun does deviate the moon from her elliptical orbit around the earth.*

These simple developments will enable us to understand the fundamental law of volume. We shall consider the four principal types one after the other.

TYPE AB. This is a simple juxtaposition with probably only a slight approaching of the two atobars A and B towards one another. Hence vol. A + B = vol. AB or for these atom-volumes: 1 + 1 = 2. After combining A and B *move as one body*, if we suppose that the atobar occupies the atomic volume in virtue of its vibratory heat-motion; or, if we accept the view of ether-envelopes, we must suppose the two atobars in perfect contact with their ether-atmospheres. The best known example is HCl; also the Kaloid-Chlorids, *Ka* Cl, or more generally the Kaloid-Chloroids, *Ka X*. The practical importance of my symbols is best understood by remembering that this formula *Ka X* embraces the following sixteen compounds, viz.: Li Fl, Li Cl, Li Br, Li Io; Na Fl, Na Cl, Na Br, Na Io; Ka Fl, Ka Cl, Ka Br, Ka Io; Rb Fl, Rb Cl, Rb Br, Rb Io.

Type AB_2, or *Water Type*. Considering the three atobars of

*It need hardly to be accentuated that the condensation here spoken of is very different from that produced by mechanical compression. In this the atobars move separately, but when brought to *one* atom they move as one body.

A, B and B, they form a plain *triangle;* and if we as a *first* approximation consider them *equal* material points, this triangle must be EQUILATERAL as the only stable figure of equilibrium $A\{^{B}_{B}$. But at all events AB_2 is *isoceles*, because each B is like the other, so that BB forms the base of this triangle. According to the difference between A and B, the triangle will deviate from being equilateral.

Let the axis from the centre of gravity of the triangle BAB to A be called z, and the other at right angles hereto in the plane AB_2 y, then we evidently have

1° for A and B considered as equal points

$$y : z = 1 : \sqrt{3} \qquad \sqrt{3} = \text{tg } 60 :$$

2° for A not being exactly equal to B, a small deviation must take place; small because the unlike atobars of A and B are always at relatively great distances from one another; hence we may put

$$y : z = 1 : \sqrt{3} + \zeta.$$

where ζ is a small, positive or negative quantity (which we will term the *deviation* or perturbation).

Now the two equal atoms B have always their centre of gravity midways between them; hence AB_2 is statically the same as atobar A and atobar 2B at the centre of gravity of B and B. In other words it is of the first type only that B_2 is represented by the centre of gravity of these B_2 or by 2B. And since now a great number of atoms B may be brought within *one* atom volume, these two B fill only one when combined, so that with the original atomic volume of A the atomic volume of AB_2 is *one volume* of A plus *one* volume B_2 giving in all *two* volumes of AB_2.

The isoceles triangle is, as it were, indicated by the typical formula $A\{^{B}_{B}$. But that this never was intended for such a purpose is well known and abundantly substantiated by the customary typical formulæ $A\{^{B}_{B}$ and even $A\left\{\begin{smallmatrix}B\\B\\B\\B\end{smallmatrix}\right.$ in our chemical works.*

*It has often been assumed that AB_2 forms a straight line *(Gaudin)*. But although AB_2 *can* form such a line, it does not retain it, heat and other vibrations would bring it out of this position, and immediately carry it farther.

TYPE AB_3. By reasoning as above we learn that this type is produced by the three B forming a triangle (equilateral).

Hence 3 vol. B are condensed to one volume, which, uniting with the one volume of A gives two volumes of the compound AB_3; the three atobars of B forming a triangle (equilateral or nearly so) above the atobar A.

TYPE AB_4 results in the same way. The four volumes of B condense to *one* by the uniting of the four atobars to one system; this one volume uniting with the one volume of A produces the two volumes of the compound. The form of the four atobars B is that of a square or nearly so; the centre of gravity of this square or of 4B constitutes as it were the new (compound) atobar which unites with A just as in the first type, without condensation, by simple juxtaposition.

In precisely the same way all other binary compounds are formed; thus in A_2B_3, the two volumes of A unite to one volume, and the three volumes of B also condense to one volume: these by juxtaposition produce two volumes of A_2B_3, wherein the three B form a triangle (equilateral or nearly so) and the two A, a straight line.

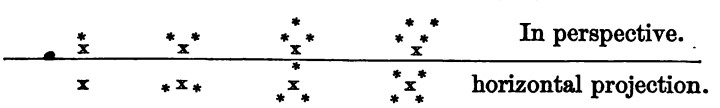
If now the compounds are to be represented by means of their symbols, we must write

$$\overset{B}{A} \qquad \overset{B\,B}{A} \qquad {}_{B}\overset{B}{A}{}_{B} \qquad {}_{B}\overset{B}{A}{}_{B}{}^{B}$$

or of the atobar B, be indicated by a star only

$$\overset{*}{A} \qquad \overset{*\,*}{A} \qquad *\overset{*}{A}* \qquad *\overset{*\,*}{A}*$$

and if also the atobar A, be indicated merely by a cross,



It will be seen, that two of the types are written by chemists in accordance with the real structure of the compound atom. The first, because two points A and B can only form a straight line; hence we have AB, or specially HCl, etc. So also the water type is written $\overset{B}{B}\}A$; for instance water $\overset{H}{H}\}O$, thus giving an exact representation of the isoceles (or equilateral) triangle formed by the three atobars.

But that no such representation was intended, is evident enough, and may also be seen from the customary method of writing the other types:

seen from above:

$\left.\begin{matrix}H\\H\\H\end{matrix}\right\}$ N instead of $_{H}{}^{H}{}_{H}N$

and $\left.\begin{matrix}H\\H\\H\\H\end{matrix}\right\}$ C instead of $_{H}{}^{H}{}_{H}{}^{H}C$

The constitution of the four types here given has been shown to be a mechanical necessity; hence experience must confirm it:

I. By the constitution and all *reactions* of the various compounds;

II. By the *physical properties* peculiar to the different compounds; and

III. By the *crystalline form* of the compounds.

In regard to the first, the *constitution* and *reactions* of bodies, we can only give one example here; *Alcohol*, C_2H_6O. Its constitution resulting from the above types (AB_4 and AB_2) is represented

Longitudinal view:

1. Vertical; $h = H_2$ 1, $^{o}{}_{h}C^{h}\,C^{h}$
2. Vertical, C = | , H = ∗ 2, 4, Front view.
3. Horizontal projection: 3,

The first formula is especially convenient; *h* is printed for H_2 because both atobars H are seen at the same spot. The horizontal projection shows some reactions plainer than the vertical projection.

In regard to *structure*, we see that each C is acted upon by four H, each O by two H; in accordance with the atomicities of O and C. The atom possesses a *stable* equilibrium. Farthermore we see that the various views in regard to its constitution are all embraced in this true form; for all were based upon *some* experimental evidence.

Thus figure 1 shows that alcohol may be considered a monoxide of $C_2H_6 = Ae$, or alcohol = Ae O.

Again, figure 3 or 4 shows that it is built according to the water type, there being in the place of one H the body HCH_2

CH_3 or empirically CH_5 = ethyl-radical; hence the type formula: $\left.\begin{matrix} C_2H_5 \\ H \end{matrix}\right\} O.$

The modifications of alcohol are also fully represented; as we shall show by a few of the most characteristic reactions.

Oxygen can act upon the terminal H_2 (fig. 1, 2, 3) which are removed, leaving

Aldehyde C_2H_4O $\quad {}^{\circ}_{h}C\,{}^{h}C$

and this vacant space will by exposure to oxygen be occupied with it, forming

Acetic acid $C_2H_4O_2$ $\quad {}^{\circ}_{h}C\,{}^{h}C^{\circ}$

By means of proper hygroscopic bodies the water atom may be removed intact from the one extremity, giving ethen (olefiant gas) and water

${}^{\circ}_{h}C^{h}\,C^{h}$	gives	${}^{\circ}_{h}$	and	$C^{h}\,C^{h}$
alcohol		water		ethen

Again, by passing the vapor of alcohol through a red hot tube, it will be decomposed, so as principally to *shake off* the terminal water H_2O and H_2, as shown by

${}^{\circ}_{h}C^{h}\,C^{h}$	gives	${}^{\circ}_{h}$	and	$C^{h}\,C$	and	^{h}
alcohol		water		acetylene		hydrogen
C_2H_6O		H_2O		C_2H_2		H_2

These few examples must be enough here; they indicate already several reactions which cannot be accounted for either by the older views, or by the type theory. The late attempts of *Frankland*, *Erlenmeyer* and others, show that such theory is urgently called for by the demands of the rapidly advancing science.

Concerning the *physical properties* time forbids to go into any details at this place; I therefore beg leave to refer to the third section of *Atomechanik*, to another paper to be presented to this Association, and to my paper on *Tyndall's* measurements of the absorption of radiant heat by gases, which values have been calculated by me on the basis of the constitution of matter here presented. (Am. Journal of Mining, May 2, 1868).

Lastly, *the* CRYSTALLINE FORM *of the compound* AB_2 *confirms the structure here given.* From the centre of gravity of the

equilateral *tritoid* AB_2 draw an axis z through A; the axis at right angles thereto is y, and since AB_2 is an equilateral triangle, we have

$$z = y \text{ tg } 60° = y\sqrt{3},$$

or

$$y : z = 1 : \sqrt{3} \qquad (1)$$

This is *the normal form*, resulting if A and B are equal, or nearly enough equal to one another that the difference between them is not perceptible at their great relative distances. But if A have a very great affinity for B, if the weight (mass) of A be very much in excess of B, then z is somewhat smaller than

$$\sqrt{3} = 1.73205 \qquad (2)$$

and will be represented by $\sqrt{3} - \zeta$, where ζ is the *deviation* or *perturbation* of z due to the deviation of A from equality with B.

By aggregation the crystallographic axes are necessarily in the same ratio as these atomic axes, so that y and z also stand for the *crystallographic axes;* for the CRYSTAL IS SIMPLY AN AGGREGATE OF ATOMS IN PARALLEL POSITIONS, so that the labors of *Bravais*, *Frankenheim* and others become directly applicable.

From the ratio here given it follows that the prism parallel to the axis x or at right angles to the plane yz must have the angles of 120° and 60°, if $\zeta = o$, or nearly this value if ζ not equal to zero. In other words the *tritoid* AB_2 *is truly hexagonal or nearly so.*

The vertical axis x is determined by simple aggregation. The *cube* being the simplest case, will give

$$x = \sqrt{\tfrac{3}{2}} = 1.22474,$$

or the form

$$x : y : z = \sqrt{\tfrac{3}{2}} : 1 : \sqrt{3} \qquad \text{A.}$$

the cube in its rhombohedral position. Striations parallel the terminal edges of the cube are characteristic of this form, and constitute an interesting proof for the correctness of this arrangement of the atoms in the crystal (Pyrites).

But we may also have aggregation simply in plane xz or xy, according to a *square;* hence either

$$x = z = \sqrt{3}$$

and the form

$$x : y : z = \sqrt{3} : 1 : \sqrt{3} \qquad \text{B.}$$

or we may have

$$x = y = 1$$

and the form

$$x : y : z = 1 : 1 : \sqrt{3} \qquad \text{C.}$$

It must not be forgotten that y in these forms has not the

same *absolute* length; but we cannot go into any detail here, and must defer this point to some other occasion. (See also *Atomechanik*, p. 31–34.)

According to the most characteristic examples, we shall call the form A: *Pyrite-form*, B: *Rutile-form*, and C: *Quartz-form*. The axis y being always taken as unity, only x and z have the deviations ξ and ζ from the normal values, due to the weight, form, etc., of the atoms A and B.

The following table contains all minerals of the type AB_2 the crystalline form of which has been determined. For the sake of simplicity, I give in this table only the name, formula, crystalline system, *vertical prism*, *i.e.* the angle between the vertical prism ∞ P (for x vertical) and the axis y; the deviation ζ of the actual form from the normal; also the deviation ξ, the latter written in the column corresponding to the aggregation being according to *Quartz* (C), *Pyrites* (A) or *Rutile* (B). At the close of the paper will be found a table giving the observations and their sources; by means of a figure for each species, the reference is made easy.

Name.	Reference.	Formula.	Crystall. System.	Vertical Prism.	Deviation.			
					$\zeta = z - \sqrt{3}$	Quartz x − 1	Pyrites, $x - \sqrt{\frac{3}{2}}$	Rutile, $x - \sqrt{3}$
I. R Fl.						ξ	ξ	ξ
Fluorspar.	1	Ca Fl Fl	tesseral	60° 0′	0.000	. . .	0.000	. . .
Yttrocerite.	2	R Fl Fl	tesseral	60° 0′	0.000	. . .	0.000	. . .
Fluocerite.	3		tesseral?	60° 0′	0.000	. . .	0.000	. . .
II. RCl_2.								
Cotunnite.	4	Pb Cl Cl	prismatic	59°19′	− 0.047	+ 0.003	. . .	. . .
III. ΘR_2.								
Copper glance.	5	S Cu Cu	prismatic	59°47′	− 0.014	+ 0.111	. . .	. . .
Stromeyerite.	6	S Cu Ag	prismatic	59°47′	− 0.014	?	. . .	. . .
Silver glance.	7	S Ag Ag	tesseral	60° 0′	0.000	. . .	0.000	. . .
Naumannite.	8	Se Ag Ag	tesseral	60° 0′	0.000	. . .	0.000	. . .
Hessite.	9	Te Ag Ag	tesseral?	60° 0′	0.000	. . .	0.000	. . .
Cuprite.	10	O Cu Cu	tesseral	60° 0′	0.000	. . .	0.000	. . .
Water.	11	O H H	hexagonal	60° 0′	0.000	?	?	?
IV. $\Sigma\delta S_2$,		with *As*	replacing	S.				
Marcassite.	12	Fe S	prismatic	59°31′	− 0.034	. . .	+ 0.049	. . .
Pyrite.	13	Fe S	tesseral	60° 0′	0.000	. . .	0.000	. . .
Hauerite.	14	Mn S S	tesseral	60° 0′	0.000	. . .	0.000	. . .
Gersdorffite.	15	Ni S As	tesseral	60° 0′	0.000	. . .	0.000	. . .
Ullmannite.	16	Ni S Sb	tesseral	60° 0′	0.000	. . .	0.000	. . .
Cobaltine.	17	Co S As	tesseral	60° 0′	0.000	. . .	0.000	. . .
Smaltine.	18	(Co, Ni) As As	tesseral	60° 0′	0.000	. . .	0.000	. . .
Mispickel.	19	Fe S As	prismatic	60°23′	+ 0.027	− 0.014	. . .	. . .
Leucopyrite.	20	Fe As As	prismatic	isomorphous with Mispickel.		. . .	. . .	. . .
Rammelsbergite.	21	Ni As As	prismatic			. . .	. . .	. . .
Glaucodote.	22	(Fe, Co) S As	prismatic			. . .	. . .	. . .
Sylvanite.	23	(Ag, Au) Te Te	prismatic	60°33½′	+ 0.043	− 0.034	. . .	. . .
Molybdenite.	24	Mo S S	hexagonal	60° 0′	0.000	?	?	?
V. RO_2.								
Cassiterite.	25	Sn O O	quadratic	56° 5′	− 0.245	. . .	. . .	− 0.245
Rutile.	26	Ti O O	quadratic	57°12½	− 0.180	. . .	. . .	− 0.180
Pyrolusite.	27	Mn O O	prismatic	57°12½	− 0.180	+ 0.066	. . .	. . .
Zircon.	28	(Si, Zr) O O	quadratic	57°21′	− 0.171	. . .	. . .	− 0.171
Quartz.	29	Si O O	hexagonal	60° 0′	± 0.090	+ 0.099	. . .	. . .
Anatase.	30	Ti O O	quadratic	60°34′	+ 0.045	± 0.000	. . .	. . .
Brookite.	31	Ti O O	prismatic	60°42′	+ 0.050	+ 0.059	. . .	. . .

A simple glance at the column of the vertical prism or the values of ζ is sufficient to show that these compounds really are hexagonal or nearly so, as required by my theory; in other words, *that the triangle AB_2 is equilateral or nearly so.*

We see also that all the orthogonal systems are represented; the tesseral 13 times, the quadratic 4 times, the hexagonal 3 times, and the prismatic 11 times: thus showing again the fallacy of certain systematists who cannot conceive of continuous variation, but see everywhere chasms and fictitious boundaries.

We have not time for a special consideration of the deviations ξ and ζ; but we cannot omit at least a few general remarks.

It is evident, that the cube is the most general position of equilibrium; indeed, if the atoms A and B were really equal material points, the cube would probably be the only form occurring in nature. We find this cube most perfectly represented in Pyrites and in Fluospar; in the former $Fe = 56$, $2S = 64$, in the latter $Ca = 40$, $2Fl = 38$, or A very nearly equal to 2B. Furthermore we notice, that the pyrites form, or $x = \sqrt{\frac{3}{2}}$ indeed is the most common.

Again, the form $x = y$, the square of the short axis is easier formed than the square of the long axis $x = z$; and accordingly we see this latter form only in 3 cases (Rutile, Cassiterite, and zircon) and even in these cases with the axis z very much shortened.* This variation can of course only be explained by reference to the composition of the elements; hence we must omit it here. But we may determine the rate of variation of ζ as dependent on the atomic weight $T\tau$ of the titanoids Sn, Ti, Zr, Si entering in the composition; for zircon we must of course take the mean of Zr and Si, according to the formula of zircon. We obtain

		$T\tau$	$-\zeta$	calcul.
Cassiterite		118	0.245	0.248
Rutile		50	0.180	0.180
Zircon	$\frac{89.6 + 28}{2}$	$= 59$	0.171	0.189

We see the last two correspond well enough. We may, for

* Also in *Tridymite* the form of SiO_2 discovered since this paper was written.

Rutile take 0.175, the mean, as the value corresponding to the atomic weight of 50. But $\frac{0.245-0.175}{118-50} = \frac{0.070}{68} = 0.001$ very nearly; hence

$$-\zeta = 0.130 + 0.001 . T\tau,$$

which gives the values above given as calculated, corresponding well enough to show that the deviation is principally proportional to the weight of the atom, *that is to the deviating mass, at the rate of* 0.001 *for each unit of the atomic weight of the element.* This formula would give for Pyrolusit the deviation $-\zeta = 0.185$ instead of 0.180 observed. For Quartz we would accordingly obtain a *quadratic* form, having $-\zeta = 0.158$ or $z = 1.574 = \text{tg}\, 57° 35'$; but this form has not yet been observed, the common form of Quartz being hexagonal, $\zeta = o$.

This rate of increase becomes of very considerable interest, when compared to that of other compounds. For the axis $z = \sqrt{3}$ we have the following deviations:

Sulphate-Series,	$-\zeta = 0.016 + 0.0008 . Xa.$
Aragonite-Series,	$\zeta = 0.163 + 0.0009 . Xa.$
Rutile-Series,	$-\zeta = 0.130 + 0.0010 . T\tau.$

or the variation of the same axis in these compounds is respectively

8, 9, 10 *ten-thousandths*

of the unit $y = 1$ for each unit in the atomic weight of the disturbing element, so that as a first approximation we have here, as in astronomy, the *disturbing force proportional to the disturbing weight or mass!*

In conclusion we must say one word in regard to the *sign* of the deviation. For the *cube* the sign of ζ and of ξ must necessarily be opposite; a contraction in one direction (—) must correspond to an expansion (+) in the other. We can test this on Marcassite, for which indeed ξ is positive, while ζ is negative. Since the cubical Pyrites has the same composition, so that the unit y for both minerals is the same absolute quantity, it follows that the atomic volume must be measured by x, y, z, or the crystallic volume; hence the *density* of these

two modifications of FeS_2 must be inversely as the product x, y, z, of their crystallographic axes. Now this product x, y, z, is

$$\frac{\text{Marcassite}}{\text{Pyrites}} = \frac{2.161}{2.121} = 1.02$$

while the density is

$$\frac{\text{Pyrites}}{\text{Marcassite}} = \frac{4.8 \text{ to } 5.0}{4.7 \text{ to } 4.8} = 1.02 \text{ to } 1.04$$

showing sufficient correspondence.

For the *other forms* the deviations must on the whole be towards the simplest position of equilibrium, that is toward the cube. Hence in the quartz form ξ must be positive, to make $y = 1$ approach toward $y = \sqrt{\frac{3}{2}}$; in the rutile-form it must be negative, for the same reason, y being $\sqrt{3}$ or greater than in the cube. By means of the table it will be seen that there are only two exceptions to this rule; the Mispickel group and Sylvanite having ξ negative, although they belong to the quartz-form, $x = 1$. But in each case it may be due to the presence of an associate element of high atomic weight: Gold ($Au = 197$) with silver ($Ag = 108$) in sylvanite, and Arsenic ($As = 75$) with sulphur ($S = 32$) in the Marcassite group.

So whether we look upon the crystalline form of the compounds of the water-type in general, or consider the deviations from the normal form in particular, we find the most perfect harmony between calculation and observation. But the more accurate calculation of these deviations can only be made upon the basis of the *composite nature of the elements.* As in astronomy it would be impossible to estimate the precession of the equinoxes and the nutation, if we would close our eyes to the fact that the earth is ellipsoidal and not a perfect sphere; so we cannot here calculate these crystallographic perturbations *unless* we admit the individual form and structure of the atoms of the elements, which according to my Atomechaniks is very far from spherical. But not only the crystalline form; also the *atomicity* of the elements require the aid of pantogen for a successful explanation.

APPENDIX.

TABLE OF THE OBSERVED VALUES MADE USE OF IN THIS ARTICLE.

No. of Species.	x.	y.	z.	Notes.
2	?	?	?	cleavage in 2 directions at angle of 108°30′.
4	$2a = 1.1906$	$c = 1.1868$	$2b = 2.000$	Prism. $i\bar{1}$.
5	$\frac{1}{2}a = 1.1116$	$b = 1$	$a = 1.7176$	
6	. . . .	. . . .	. . . .	isomorphous with No. 5.
12	$b = 1$	$\frac{a}{2} = 0.7868$	$c = 1.3287$	Prism. $\frac{1}{2}\bar{1}$.
19	$\frac{1}{2}c = 0.9862$	$b = 1$	$a = 1.7588$	Prism. $1\bar{1}:1\bar{1}$.
23	$\frac{1}{2}c = 0.9664$	$b = 1$	$a = 1.7721$	Prism. $1\bar{1}:1\bar{1}$.
25	$\frac{1}{\bar{a}} = 1.487$	$\frac{a}{\bar{a}} = 1$	$\frac{1}{\bar{a}} = 1.487$	$1i:1i = 112°10'$.
26	$\frac{1}{\bar{a}} = 1.552$	$\frac{a}{\bar{a}} = 1$	$\frac{1}{\bar{a}} = 1.552$	$1i:1i = 114°25'$.
27	$c = 1.066$	$b = 1$	$2a = 1.552$	
28	$\frac{1}{\bar{a}} = 1.561$	$\frac{a}{\bar{a}} = 1$	$\frac{1}{\bar{a}} = 1.561$	$1i:1i = 114°42'$.
29	$a = 1.0999$	$b = 1$	$b \checkmark 3$	$0:\frac{1}{2}i = 150°42'$.
30	$c = 1$	$b = 1$	$a = 1.7723$	$1i:1i = 121°8'$.
31	$a = 1$	$b = 1.05889$	$2c = 1.78228$	e of *v. Kokscharow*.

All these values, with the exception of the last (No. 31) are taken from *Dana's* Mineralogy, fourth edition. The hexagonal and particularly the tesseral forms are not here enumerated, since the system gives the numerical values.

The reading of this article was illustrated by numerous models and diagrams.

---

24. A New and General Law Determining the Atomic Volume and Boiling Point of a Great Number of Carbon Compounds. By Professor Gustavus Hinrichs, Iowa State University.

From the atomic constitution of a very great number of organic compounds, especially the oxides of the hydrocarbons, I have deduced a very simple and general law of the *atomic volume;* this law being confirmed by observation, will accordingly be considered an additional proof for the correctness of the constitution of compounds as given in my *Atomechanik*, 1867. The new law for the atomic volume is:

Compounds of n carbon atoms, these being joined by pairs of

hydrogen-atoms and provided with a terminal water atom H_2O, *possess the atomic volume*

$$v_n = 1 + n,$$

the atomic volume of water being taken as unity.

If a, the atomic weight ($O = 16$, $H = 1$, etc.), and s, the specific gravity observed, the resulting or OBSERVED

$$\textit{atomic volume} \text{ is } \quad v = \frac{a}{18.s} \quad \text{for any body.}$$

In comparing these observed values v, with the values v_n, calculated by means of the above law, it must be remembered, that the atomic volumes are only strictly comparable at the boiling points; hence if the coefficient of expansion of the liquid be α (for each degree C) and the *difference* between the boiling point T and the temperature t at which the specific gravity s is determined be $\tau = T - t$, then the volume v at t would become

$$v' = v(1 + \alpha\tau)$$

In other words, the lower the temperature t at which s is determined below the boiling point of the liquid, the *greater* the increase which the volume would suffer by heating the substance to its boiling point. We shall now proceed to the consideration of some of the groups which come under the above law; and from these examples the law itself will become better understood.

I. THE MONATOMIC ALCOHOLS.

Their empirical formula is $C_nH_{2n+2}O$; their true constitution is represented by the longitudinal elevation of one atom:

$$\overset{\circ}{{}^{h}C^{h}}\,C^{h}\,C^{h} \ldots\ldots {}^{h}C^{h}\,C^{h}\,C^{h} \qquad (n \text{ atoms of C})$$

where each h indicates the two atoms of hydrogen projected at the spot h. From this formula or figure it is apparent, what we mean by the pairs of hydrogen-atoms ($h = 2H$) tying together the carbon-atoms (C), and $hO = H_2O$ is seen as a "terminal" to the alcohol-atom. Whether now this is the true atomic constitution, or not, we may leave an open question here; it will be sufficient to show that the above law of atomic volume results from it, *and that experience does confirm this law!*

In this atom there are n equal terms C^h united in the same manner to form a prism; let the volume occupied by any one C^h be x, then the volume of the entire prism $C_n H_{2n}$ will be nx. The atom water ($H_2O = {}_hO$) when free has the volume $= 1$. Its volume in this liquid compound may therefore also be taken as *one*. The constitution of $C^h = CH_2$ and the terminal OH_2 is the same; it is therefore probable that the space they occupy is the same, or that $x = 1$. Hence the entire volume of the atom $C_nH_{2n+2}O$ will be $1 + n$ as above stated.

We now give the observed values of the specific gravity s the observed atomic volume v deduced therefrom; by comparing this with the calculated value v_n we determine the deviation d in per cent. of v_n. We finally add the boiling point T (in centigrades).

Alcohol,	n	s	v	v_n	d	T
Methyl,	1	0.81	2.18	2.00	+9	66°
Ethyl,	2	0.79	3.12	3.00	+4	78.4°
Butyl,	4	0.89	5.10	5.00	+2	109°
Propyl,	5	0.825	5.94	6.00	−1	132°
Octyl,	8	0.823	8.73	9.00	−3	180°

Since now s is determined at about the same temperature, this temperature is comparatively *near* the boiling point of the lower members (n small), but far below the boiling point for the higher members of the series. In other words, the determinations of v for the lower members is too high, as compared to that of the higher members. Indeed we see the deviations of observation minus calculation *positive* for the lower members, *negative* for the higher members, and regularly decreasing from the first to the last. By means of our formula, before given, we can readily see that the deviations must be of the

form $d = h + k\,T$;

and indeed by $d = 12 - 0.1\,T$

we get

n =	1	2	4	5	8
d observed =	+9	+4	+2	−1	−3
d calcul. =	+6	+4	+1	−1	−6

almost identic, or showing that our law $1 + n$ indeed is the true law for the atomic volume of the monatomic alcohols.

This influence of the temperature is also well shown in the case of ethyl-alcohol, C_2H_6O. At 4°C its specific gravity is 0.81, hence its atomic volume $v = 3.15$; but at 10° C its specific gravity 0.79 gives the volume 3.23, an increase of 0.08 for an increase of 6°C in temperature. Hence for a limited range of temperature the atomic volume of alcohol is at the temperature t equal to

$$v_t = 3\ [1.032 + 0.0044\, t]$$

or $v_t = 3$ exactly at about $-7°$ C. This is accordingly 85° below its boiling point, so that if we adopt the general view that liquids may properly be compared at equal distances below their respective boiling points, the densities if determined at the following temperatures

	−71°	−7°	+24°	+47°	+95°
for n	1	2	4	5	8°

the resulting atomic volumes would be *exactly* equal to the theoretical values

2 3 5 6 9.

II. Acids of the Monatomic Alcohols.

The empirical formula of these acids is $C_n H_{2n} O_2$. They are derived from the alcohols by replacing the terminal $h = H_2$ by O; or

$$^{o}_{h}C^{h}\,C^{h}\,C^{h} \ldots\ldots {}^{h}C^{h}\,C^{h}\,C^{o} \qquad (n \text{ atoms C})$$

The atomic volume is the *same* as for the corresponding alcohol. — We have:

Acid	n	s	v	v_n	d	T
Formic	1	1.235	2.07	2.00	+3	100°
Acetic	2	1.063	3.12	3.00	+4	118°
Propionic	3	0.991	4.12	4.00	+3	140°
Butyric	4	0.974	5.015	5.00	+0.3	162°
Valerianic	5	0.938	6.030	6.00	+0.5	174°
Capronic	6	0.931	6.916	7.00	−1.3	199°
Lauric	12	0.883	12.604	13.00	−3.3	?

These figures need no commentary; the remarks above made apply also in this case.

III. Different Compounds of equal Atomic Volume.

Quite a number of such serial compounds, of like atomic structure, but *very different empirical formula*, will accordingly have the same atomic volume, if only they contain the same number of carbon atoms. We shall consider several cases with 1, 2, 3 carbon atoms, and also some monamines.

A. $n = 1$; hence *atomic volume* $= 1 + n = 2$.

		s	v	v_n	d	T
Formic acid,	CH_2O_2	1.235	2.07	2.00	+3.5	100°C
Methyl alcohol,	CH_4O	0.814	2.18	2.00	+9	66°

The deviation for the latter, with a much lower boiling point, is much greater than for the former, in accordance with the preceding explanation.

B. $n = 2$; hence *atomic volume* $1 + n = 3$.

		s	v	v_n	d	T
Aldehyde	C_2H_4O	0.801	3.05	3.00	+1.6	21°
Glycol	$C_2H_6O_2$	1.125	3.06	3.00	+2	197.°5
Acetic acid	$C_2H_4O_2$	1.063	3.13	3.00	+4	117°
Alcohol	C_2H_6O	0.81	3.15	3.00	+5	79°

It appears from this that with the exception of aldehyde, all deviations are so much the smaller, the higher the boiling point, in conformity with preceding explanations. But the case of aldehyde does not represent any anomaly; for my theoretical formula is

$$\text{Aldehyde } {}^{\circ}_{h}C^{h}C$$

or being devoid of the terminal $h = H_2$, and correspondingly this compound has at common temperatures a somewhat smaller atomic volume.

For the latter three it is seen that an increase of 40° C in the boiling point diminishes the volume about one per cent.; hence the deviation for aldehyde ought to be about + six per cent., while it really is only about + two per cent. Hence the removal of the terminal $H_2 = h$ corresponds to only about four per

cent., or $\frac{1}{25}$ of the unit of volume, the volume of one atom of water.

C. $n = 3$; hence *the atomic volume* $= 4$.

		s	v	v_n	d
Glycerine	$C_3H_8O_3$	1.28	4.00	4.00	$0._0$
Lactic acid	$C_3H_6O_3$	1.215	4.11	4.00	-3
Propionic acid	$C_3H_6O_2$	0.991	4.15	4.00	$+4$

D. MONAMINES have the same mechanical structure as the alcohols, except that the terminal water H_2O is replaced by ammonia H_3N. Hence since $H_3 : H_2 = 3 : 2$, the former H^3 forming an equilateral triangle each side of which is nearly equal to the distance between H and H in water H_2O, it follows that the atomic volume of NH_3 must be determined by

$$H_3N : H_2O = 3 : 2 \text{ or vol. } H_3N = 1.5$$

and hence the volume of the monamines $C_n H_{2n+3} N = NH_3 + n CH_2$ must be

$$v_n = 1.5 + n$$

This is fully confirmed by the observations at my disposition; these give

	n	s	v	v_n	d
Ammonia	0	0.62	1.52	1.50	$+1$
Ethylamine	2	0.696	3.59	3.50	$+3$
Amylamine	5	0.750	6.44	6.50	-1

The correspondence between calculation and observation is indeed most remarkably close.

E. GRAPHICAL REPRESENTATION. Let the square represent one atom of (liquid) water, then according to the above, the following will be a graphic and geometrically true representation of the atomic volume and constitution of the compounds enumerated in D.

	Name.	*Volume.*
$\begin{array}{\|c\|}\hline {}^{o}_{h} \\ \hline \end{array}$	Water	1.
$n = 1$		
$\begin{array}{\|c\|}\hline {}^{o}_{h}\ C^{o} \\ \hline {}^{o}_{h}\ C^{h} \\ \hline \end{array}$	Formic acid	2
	Methyl alcohol	2

		Name.	*Volume.*
$n=2$			
$\overset{o}{h}$ C^h	C	Aldehyde	3
$\overset{o}{h}$ C^h	C^h	Glycol	3
$\overset{o}{h}$ C^h	C^o	Acetic acid	3
$\overset{o}{h}$ C^h	C^h	Alcohol	3
$n=3$			
$\overset{o}{h}$ C^h C^h	C^h	Glycerine	4
$\overset{o}{h}$ C^h C^h	C^o	Lactic acid	4
$\overset{o}{h}$ C^h C^h	C^o	Propionic acid	4

This table might be extended; but we only shall observe here once more and explicitly, that the transverse section of all these compounds is the same, viz.,

$$\overset{O}{\underset{H}{\overset{|}{C}}}\!\!<\!\!\overset{}{}\!\!>\!\!\underset{}{H}$$

and hence the volume of this prism, or the atomic volume simply measured by $1+n$. See *Atomechanik*, § 123, p. 18.

Even Sucrose, $C_{12}+11\,(H_2O)$ gives the atomic volume $v=11.83$ from the observed value of its specific gravity (1.606); now, since no H_2O is terminal, we may have according to the above principle, $v_n=n=12$ which is only 0.17 or $1\frac{1}{2}$ per cent. greater than the observed value. There being no terminal water, the volume is not $1+n$ but simply n.

Atomic Volume of the Elements.

According to *Atomechanics*, the constitution of the element-atoms is in so far similar to the constitution of the preceding carbon compounds that the element-atoms consist of a *series of plates* (called *atomares*) which may be compared to the consecutive links $CH_2=C^h$ of the carbon series. A direct proof therefore we have in the circumstance that these *atomares* in the same group of elements occupy nearly the same volume, or are *isostere*.

If the atomic volume of an element be v (water $=1$) and

the number of *atomares* in the atom be m (*atometer*), then the volume of each atomare of this element will be

$$w = \frac{v}{m}.$$

Taking the values of m as given in my Atomechanics, we find for

PHOSPHOIDS Φ:	v	m	w	F
P	0.94	9	0.10	44°
As	0.72	8	0.09	?
Sb	1.00	13	0.08	450°
Bi	1.14	12	0.09	265°

Here the influence of the fusing point F will fully account for the slight variation; w for Sb is the smallest, because its volume is taken farthest below its fusing point; that of P is the largest, because taken nearest its fusing point.

CHLOROIDS, X:	v	m	w
Cl	1.48	13	0.113
Br	1.49	13	0.114
Io	1.42	12	0.118
mean			0.115

		obs.	p.	*calc.*	*diff.*
CADMOIDS, $K\delta$:	Mg	— 0.76	3	0.75	+ 0.01
	Zn	0.51	2	0.50	+ 0.01
$v = p.K$	Cd	0.71	3	0.75	— 0.04
	Pb	1.00	4	1.00	0.00

The cadmoids accordingly follow the same law, a number p multiplied into the same constant, giving the atomic volume v inside the errors of observation. We may accordingly consider, by comparing this result with the law governing the carbon series, that one atom of

Zink	consists	of	2	atomares
Cadmium	"	"	3	"
Lead	"	"	4	"
Magnesium	"	"	3	"

which coincides with the formula given in atomechanics, viz.,

$Zn = A + B$	where	$A = 2.4^2$ or [4.4]
$Cd = A + 2B$	"	$B = [8.12]$
$Pb = A + 3B$		
$Mg = 3A$	"	$A = [4.4]$

so that vol. A = vol. B = 0.25 or $\frac{1}{4}$ of one atom of water.

KALOIDS, *Ka*:		*v*	*p*	*calc. v.*	*diff.*
	Li	0.65	1	0.63	+0.02
$v = p.K$	*Na*	1.32	2	1.26	+0.06
$K = 0.63$	*Ka*	2.50	4	2.52	−0.02
	Rb	3.11	5	3.15	−0.04

thus plainly indicating that the successive Kaloid atoms contain 1, 2, 4, 5 or $q, 2q, 4q, 5q$ atomares. This result differs from the formula given in Atomechanik, §25. We may therefore conclude, that this preliminary formula, $Ka = 2(7) + 2m.4^2$, given in Atomechanics is not the true formula, and does not quite correctly express the atomic constitution of the Kaloids. The values of m are respectively: *Li*, 0; *Na*, 1; *Ka*, 2; *Rb*, 5.

CONCLUSIONS.

The law for the atomic volume of serial compounds is very different from what chemists have been led to believe by *Kopp* and others; for we cannot, from the empirical formula, calculate the volume by giving a certain fixed value to each constituent atom, as *Kopp* has attempted, but the volume is simply that of the PRISMATIC ATOM or the product of maximum transverse-section into altitude or length of the atom. The section for the atoms here considered has been equal to that of a water atom; and the unit of length equal that of a water atom also, so that the volume of the compound atom, or

$$1 + n$$

is simply the *length* of the atom, that of water being 1. Thus the four different compounds given under III, *B*, have all the same atomic volume, and yet if we put $C_2H_4O = A$, we have empirically

Aldehyde	$= A +$ zero
Glycol	$= A + H_2O$
Acetic acid	$= A + O$
Alcohol	$= A + H_2$

from which by *Kopp's* reasonings we would have to conclude that the atomic volume of water, H_2O is the same as that of oxygen O, or of two hydrogen H_2 and the same as nothing, hence that H_4O in A does not occupy any space, and that the

volume is exclusively determined by carbon; but then this is manifestly wrong, since

vol. water $H_2O = 1$ and not zero!
vol. methyl-alcohol $CH_4O = 2$, hence $C = 2$ (if $H = O =$ zero)
vol. ethyl alcohol $C_2H_6O = 3$ and not $2 \times 2 = 4$

which would follow from the above. We therefore see that the current reasoning from merely empirical formula leads into a labyrinth of contradictions; it may not be presumptuous to say, that our theoretical formula, based upon the atomechanical constitution of compounds, have the criterion of truthfulness, both in the simplicity of the law and the perfect harmony between the same and observation.

And finally, since we find that precisely the same law of atomic volume governs the elements, the conclusion appears well founded, that the *element atoms also are prismatic bodies, built of lamellæ or atomares.* This lamellar constitution is now proved by 1° the atomic weight; 2° the refraction-equivalents of the molecular-physicist and crystallographer, *Schrauf;* 3° by the absorption of radiant heat in gases, simple and compound, measured by *Tyndall;* 4° boiling and fusing points; 8° length of crystallographic axes; 6° atomicity, besides the (7°) atomic volume here spoken of. For details on these points see my *Atomechanik*, 1867, and on 3° see my paper,—Atomechanics proved by *Tyndall's* Experiments in the American Journal of Mining, May 2, 1868.

And from all this we again affirm the composite nature of the so-called elements, and predict that they first will be decomposed into these lamella or atomares; finally the latter will be resolved into panatoms.

The Boiling Point of the Isomers $C_6H_{14}O$.

The general law in regard to the boiling point referred to in the title of this paper is *the boiling point depends on the form of the atom; the more symmetric the atom, the lower the boiling point; and the reverse.*

We shall now illustrate it first by the isomers of the empirical formula $C_6H_{14}O$; thereafter by the isomeric amines.

The compounds $C_6H_{14}O$ are monoxides of hexan C_6H_{14}, the constitution of which is

hexan $^{h}C^{h}\,C^{h}\,C^{h}\,C^{h}\,C^{h}\,C^{h}$

side view. horizontal projection.

In these figures h stands for H_2 at the spot where both H are projected; a star for H, a bar for C. Now each of the places h or H_2 can unite with the oxygen atom to form water H_2O. There are seven pairs of H or seven h, so that the oxygen atom may be united at seven different places. But two and two of these places produce the same atomic body, and hence the same compound. If the atom of hexan be represented by P_6 (protypoid of six carbon atoms) the *monoxide* may be represented by the symbol

$$_{m}\,P_{(6-m)}$$

if m indicate the number h to the left of the oxygen atom, and consequently $6-m$ the number to the right. But it is evident that

$$_{m}P_{(6-m)} \quad \text{and} \quad {}_{(6-m)}P_{m}$$

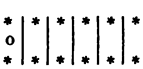
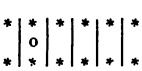
represent the same atoms seen from two different sides. Hence the seven monoxides

$$_{0}P_{6} \quad {}_{1}P_{5} \quad {}_{2}P_{4} \quad {}_{3}P_{3} \quad {}_{4}P_{2} \quad {}_{5}P_{1} \quad {}_{6}P_{0}$$

are only representing the following FOUR *really different isomers*, viz.,

$$_{0}P_{6}^{\bullet} \qquad {}_{1}P_{5} \qquad {}_{2}P_{4} \qquad {}_{3}P_{3}$$

These four compounds, all corresponding to the empirical formula $C_6H_{14}O$ are represented in the following table by their *symbol*, vertical and horizontal projection, and the typical formula; and it will readily be seen that the latter results from my horizontal projection, by simply drawing a line obliquely through the oxygen atom.

		PROJECTION.		
Name.	*Symbol.*	*vertical.*	*horizontal.*	*Typical formula.*
Hexyl-Alcohol,	$_{0}P_{6}$	$^{\circ}{}_{h}C^{h}\,C^{h}\,C^{h}\,C^{h}\,C^{h}\,C^{h}$		$\left.\begin{matrix}C_6H_{13}\\H\end{matrix}\right\}O$
Methyl-Amyl-Ether,	$_{1}P_{5}$	$^{h}C^{\circ}_{h}\,C^{h}\,C^{h}\,C^{h}\,C^{h}\,C^{h}$		$\left.\begin{matrix}C_5H_{11}\\CH_3\end{matrix}\right\}O$

		Projection.		
Name.	*Symbol.*	*vertical.*	*horizontal.*	*Typical formula.*
Ethyl-Butyl-Ether,	${}_2P_4$	$^{h}C^{h}C^{h}\overset{\circ}{C}{}^{h}C^{h}C^{h}C^{h}$	$\begin{matrix}*&*&*&*&*&*&*\\ *&*&\circ&*&*&*&*\end{matrix}$	$\left.\begin{matrix}C_4H_9\\ C_2H_5\end{matrix}\right\}O$
Propyl-Ether,	${}_3P_3$	$^{h}C^{h}C^{h}\overset{\circ}{C}{}^{h}C^{h}C^{h}C^{h}$	$\begin{matrix}*&*&*&*&*&*&*\\ *&*&*&\circ&*&*&*\end{matrix}$	$\left.\begin{matrix}C_3H_7\\ C_3H_7\end{matrix}\right\}O$

From these graphic representations of the mechanical constitution of the atoms it will readily be seen, that ${}_3P_3$ is the most symmetric atom of all; indeed it is perfectly symmetric, so that if we could suspend it by the oxygen atom, the whole atom would be perfectly balanced. Again the alcohol ${}_0P_6$ is the least symmetric of these four isomers.

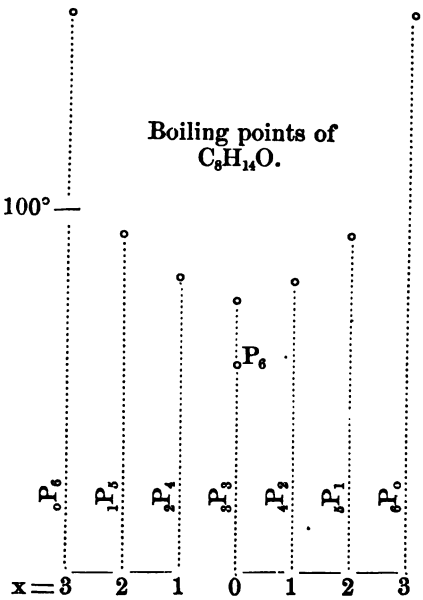
According to the law above given we conclude: *the boiling point of these isomers* $C_6H_{14}O$ *is* LOWEST *for* ${}_3P_3$, *highest for* ${}_0P_6$ *and increasing according to some definite law from* ${}_3P_3$ *to* ${}_0P_6$.

If x denote the abscissa, the distance from the central $h = H_2$, and t_x the corresponding boiling point (centigrade) we have indeed

$$t_x = 76° - x + (4.25)^x$$

as the law expressing the variation of the boiling point for these isomers. That this law expresses the observed values we see from the following table:

Symbol	x	*t calc.*	*t obs.*
${}_0P_6$	3	149°.76	150°
${}_1P_5$	2	92°.06	92°
${}_2P_4$	1	79°.25	80°
${}_3P_3$	0	76°	76°



where the boiling point for ${}_3P_3$ has been determined by interpolation from the other simple ethers. These results are represented in the adjoining figure, on the scale of

$x = 1 = 1$ centimeter
$t = 1°C = \frac{1}{2}$ millimeter.

It is evident that a like law, but with constants depending on n corresponds to the isomeric compounds of any group of isomers expressed by

$$C_n H_{2n+2} O.$$

The Boiling Point of the Amines.

The amines are very rich in isomers and therefore eminently adapted to exemplify the law above given. By means of the horizontal projection we can readily represent the atomic constitution of the amines. They may all be considered as derivations of ammonia NH_3

represented here in horizontal projection. The three sides HH may each grow by the addition of any number of $C^h = CH_2$, so that the general amine

$$\left.\begin{matrix} C_p H_{2p+1} \\ C_q H_{2q+1} \\ C_r H_{2r+1} \end{matrix}\right\} N$$

Symbol:

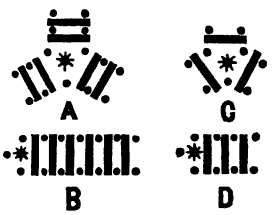
results by the prisms $C_pH_{2p} = p\,(CH_2)$, $q\,(CH_2)$ and $r\,(CH_2)$ being added on the three sides of NH_3 formed by the hydrogen atoms two and two. The symbol here given accordingly needs no farther explanation.

If now $p = q = r$, the amine is the *most symmetric* that can be formed by means of the same number of carbon atoms; it will, from among all the isomers represented by

$$C_{(p+q+r)}\,H_{2(p+q+r)+3}N$$

have the *lowest* boiling point.

In the adjoining figure



C = Trimethylamine $= {}_1\overset{1}{N}_1$

A = Triethylamine $= {}_2\overset{2}{N}_2$

B = Hexylamine $= {}_0\overset{0}{N}_6$

D = Propylamine $= {}_0\overset{0}{N}_3$

The observed boiling points confirm our law, as may be seen from the following table of *boiling points*, wherein isomers are in a horizontal line:

	Monamines.		Diamines.		Triamines.	
1	${}_1\overset{0}{N}_0$	below 0°				
2	${}_2\overset{0}{N}_0$	18°.7	${}_1\overset{1}{N}_0$	8°.5		
3	${}_3\overset{0}{N}_0$	49°.7	${}_2\overset{1}{N}_0$	?	${}_1\overset{1}{N}_1$	4°–5°
4	${}_4\overset{0}{N}_0$	69°	${}_2\overset{2}{N}_0$	57°.5	. . . . .	
5	${}_5\overset{0}{N}_0$	94°	. . . . .		. . . . .	
6	${}_6\overset{0}{N}_0$	126°	. . . . .		${}_2\overset{2}{N}_2$	91°
7	${}_7\overset{0}{N}_0$	146°	. . . . .		. . . . .	
8	${}_8\overset{0}{N}_0$	170°	. . . . .		${}_1\overset{2}{N}_5$	135°
9	. . . . .		. . . . .		${}_2\overset{2}{N}_5$	154°
10	. . . . .		${}_5\overset{5}{N}_0$	170°	. . . . .	
15	. . . . .		. . . . .		${}_5\overset{5}{N}_5$	257°

This table needs no commentary. We see indeed that

the more symmetric an atom, the lower is the boiling point; for

$_2\overset{o}{N}_o$	by	10°	greater than	$_1\overset{1}{N}_o$
$_3\overset{o}{N}_o$	by	44°	" "	$_1\overset{1}{N}_1$
$_6\overset{o}{N}_o$	by	·35°	" "	$_2\overset{2}{N}_2$

According to the law

$$t_p = 25° p - 31°$$

expressing the boiling points of the monamines

$$_p\overset{o}{N}_o$$

with great accuracy, as may be seen from

		Boiling point	
p	*Name*	*calcul.*	*observed.*
1	Methyl-amine	— 6°	below 0°
2	Ethyl "	+ 19	+ 18°.7
3	Propyl "	44°	49°.7
4	Butyl "	69°	69°
5	Amyl "	94°	94°
6	Hexyl "	119°	126°
7	Heptyl "	144°	146°
8	Octyl "	170°	170°

the boiling point of $_{15}\overset{0}{N}_0$ would be 344°, while the isomeric tri-amyl-amine $_5\overset{5}{N}_5$ is 257° or 87° lower!

Thus we may consider it a fact, that greater symmetry or equilibrium of an atom is accompanied with *lower* boiling point. The cause of this law is palpable. For when boiling, the liquid is converted into vapor of equal tension with the pressure on the liquid; but vapors and gases are known to be characterized by the atoms or particles moving equally in all directions. Now it will require a greater momentum, a greater power (hence a higher degree of a heat) to make the more unsymmetric body move equally well in all directions, than the more symmetrical body. Substituting heat for motion, and atom for body, we have our law in regard to the boiling point of isomeric bodies. — See also *Atomechanik*.

We have in this paper made use of deduction rather than of induction, because the method is much more lucid and satisfac-

tory. The test remains the same: agreement with observation, and discovery of new laws and relations.

But if we had the opportunity we would herewith connect an *induction:* we would point out similar laws in the boiling and fusing points of the elements, and hence conclude their constitution to resemble that of the compounds here treated of. But since chemists have not yet realized that mathematics and observation must go hand in hand, in order to arrive at results comparable in universality to those worked out in astronomy and physics, it may be best not to push these conclusions at this time. If I have succeeded in showing that mechanical chemistry is a field as important and as grateful as any field now in scientific culture, I have attained my aim.

---

25. ATOMIC MOTIONS. By Professor H. F. Walling, of Easton, Pennsylvania.

(ABSTRACT.)

COMMENCING with the generally admitted proposition that all physical phenomena are made up of motions of matter, either "molar" or "molecular," new definitions of force and matter were advanced, drawing between them the line of demarcation more clearly and sharply than is done in the treatises on natural philosophy.

Matter was defined as *that which may be moved by becoming associated with force. Force as that which when associated with matter causes it to move.* We are justified in recognizing the substantive existence of force, as well as that of matter, and one object of this paper will be to show that the former may and does have an independent existence. Some of the so-called "primary properties" of matter, such as impenetrability, hardness, elasticity, etc., are not real properties of matter *per se*, but the phenomena upon which the supposition of their existence is based are manifestations of associated force and consequent motion.

Inertia is not a property of matter any farther than it expresses the principle that matter alone will not move. When matter and force become *associated*, the result is motion, and

since they do not spontaneously dissociate, this motion will be continuous. Opposing forces acting upon the same matter produce, if exactly equal and opposite, resultant rest.

The vexed question of the *infinite divisibility of matter* may be disposed of by admitting the mathematical principle upon which the differential calculus is founded, namely, that there are different orders of infinity both in the direction of immensity and of minuteness, the relations of quantities in the different orders being those which are considered to exist between finite and infinite quantities and between differentials of different orders. Thus chemical atoms, though infinitesimal when compared with finite bodies, have definite relative magnitudes when compared with each other, and may even be considered as infinitely great, when their diameters are compared with those of the lines of force by which they are traversed.

The postulates of the hypothesis now offered are,—

First: Force is omnipresent, occupying every possible straight line in both of its opposite directions.

Second: It is infinite in quantity in each and every line and direction.

Third: The amount of force which becomes associated with a given atom in a unit of time bears a constant ratio to the entire amount of force which acts upon the atom.

Fourth: When two atoms are situated in the same line of force there is an inequality of action producing results analogous to those which would follow the *interception* of a stream of matter or a succession of waves. The amount of force acting upon the second atom in the order of the force's direction, is less than that acting upon the first by an amount which will depend upon the magnitude or associating capacity of the latter. The word *interception* will, for convenience, be used hereafter to indicate the action here supposed.

Admitting these postulates, it will be seen that a single atom in space would be acted upon by equal forces in all directions and no motion would result. If, however, a second atom be introduced, mutual interception of force takes place in accordance with the fourth postulate, and resultant forces become mutually and simultaneously associated, urging the atoms towards each other.

The amount of force thus associated is always necessarily equal and opposite in the two atoms, for if we indicate the two atoms by A and B, and the amounts of force intercepted by each, respectively, in a unit of time from the external and opposite lines of force, by a and b, we have the resultant impelling A towards B,

$$a + \infty - (\infty - b) = a + b$$

while that impelling B towards A is

$$b + \infty - (\infty - a) = b + a$$

The law of the inverse squares is an obvious consequence of the assumption that atoms have definite size when compared with lines of force, being the law of radiant forces. For if the atoms are capable of subtending angles made by force lines, these angles will vary with the distances, and the integral of all the force rays which may pass through one atom to any point upon the other will be inversely as the square of the distance between the atoms.*

While this hypothesis of the cause of gravitation obviates the necessity for attributing an inherent attractive power to matter, enabling each atom to exercise an incredibly wonderful power of discriminating the positions of other atoms and of acting "where it is not," it likewise removes a most serious difficulty in reconciling the conservation theory with the facts in regard to gravitation. This difficulty cannot be better stated than in the words of Faraday, as follows: "There is one wonderful condition of matter, perhaps its *only* true indication, namely *inertia*, but in relation to the ordinary definition of gravity, it only adds to the difficulty. For if we consider two particles of matter at a certain distance apart, attracting each other under the power of gravity, and free to approach, they will approach; and when at only half the distance, each will have had stored up in it, because of its *inertia*, a certain amount of mechanical force. This must be due to the force

*This view of the cause of gravitation was advanced by the writer in a contribution to Silliman's Journal in 1864, vol. xl, p. 254. It was not until I read a very interesting paper by James Croll, in the "London Edinburgh and Dublin Philosophical Magazine," December, 1867, that I became aware that somewhat similar views had been previously entertained by Faraday, Waterston and others.

exerted, and if the conservation principle be true must have consumed an equivalent proportion of the cause of attraction, and yet according to the definition of gravity, the attractive force is not diminished thereby, but increased fourfold, the force growing up within itself the more rapidly, the more it is occupied in producing other force.

On the other hand if mechanical force from without be used to separate the particles to twice their distance, this force is not stored up in momentum or by *inertia*, but disappears; and three-fourths of the attractive force at first disappears with it. How can this be?"

I apprehend that the principle of conservation of energy, if applied only to *associated* force, that is, to the aggregate momentum or *vis viva* of matter in mechanical or molecular motion, can only be maintained by including in the estimate of *vis viva* an imaginary quantity called "potential energy," a conception which seems to me liable to every objection which can be brought against that of the independent existence of force, without the simplicity of the latter.

The harmony of my hypothesis with the conservation of force is evident. The equal and opposite forces which are stored up in the approaching bodies are taken from the lines of force in which the bodies are located, diminishing their quantities by just the amount which becomes associated with the bodies. When gravitation is counteracted by some other force and the approaching bodies are stopped and separated, the associated force is dissociated and restored to the line of force which is traversed.

An immensely important principle, a corollary to that just stated and forming one of the fundamental laws or axioms of dynamics, is here developed. Assuming, as I do, that the universal force already described is the only force in nature, it follows that no two atoms or bodies in the universe can, by mutual action, approach each other, without, by such mutual action, storing up equal amounts of force in opposite directions, nor can they separate without losing or imparting equal amounts of their associated force to the traversed line of force. When followed out to its results in all directions we shall perceive that this principle is equivalent to, or rather identical

with, the "third law" of Newton, namely, that action and reaction are equal and opposite.

In order to see this more clearly it will be necessary to consider the nature of what are called *repulsions*. There is no more ground for maintaining the existence of *inherent repulsions* than of *inherent attractions*. I will now attempt, therefore, to show how bodies or atoms are impelled *from* each other.

The obvious and simple cause of this *apparent* repulsion is the momentum or *inertia*, of the matter in motion. A comet for example, whirls with almost incredible velocity around the sun, and flies off into space in a nearly parabolic orbit. Is repulsion the cause of this? No more than it is the cause of the continued ascending motion of the pendulum after it has passed its lowest point. The true cause in both cases is the momentum of the moving body. This is familiar to every student of astronomy or mechanics.

By applying the same principle to the motions of atoms we shall see that the ultimate or limiting force with which atoms or molecules will separate from each other is equal, in any pair, to the excess, at any position, of the actual momentum over that which would have been acquired in falling by mutual gravity, towards each other to that position from an infinite distance. This excess constitutes, therefore, the force of virtual repulsion.

If now we free our minds from the dogmas of the impenetrability of matter, hardness of atoms, etc., we may obtain wonderfully simple and satisfactory views of the constitution of matter, the transformations it undergoes and the function of heat in effecting these transformations.

Let us first consider the nature of heat, the cause of its "repulsive agency," and the reasons for the changes of condition from the solid to the liquid and from the liquid to the gaseous condition, which continued additions of heat produce upon matter.

If we suppose the atoms of all bodies to be in motion among each other, impelled towards each other by mutual gravitation, and moving beyond each other in accordance with the law of *inertia*, we may give the name heat to the atomic motion, and

temperature will be represented by the momentum, or MV. We shall see that there may be three conditions of motion in a body or aggregation of atoms.

First: That in which the acquired motion is less than the potential motion of gravity; that is, less than would be acquired by falling from an infinite distance. In this case the orbits of the atoms would be such as to retain the aggregation within finite limits, corresponding to elliptical orbits of the solar system. This would be the *solid* condition of matter.

Second: That in which the acquired motion is just equal to the potential of gravity, or what would be acquired by falling an infinite distance. The "attractions" and "repulsions" would, in this condition be exactly balanced and the condition one of unstable equilibrium, represented by the surface of a volatile *liquid*, where, doubtless, vaporization and condensation are continually going on, as determined by minute variations of temperature and pressure.

Third: That in which the acquired motion exceeds the potential of gravity, when virtual repulsion takes place; that is, the atoms tend to separate to an infinite distance with a positive force, giving rise to the *gaseous* condition.

The problem of tracing deductively the paths of innumerable atoms, each attracting all the others according to the law of the inverse squares, seems one of hopeless complexity, and probably exceeds the power of the human intellect. It is only by inductive processes that we may hope to arrive at useful results in this direction. The problem is simplified very greatly by the chemical law of the absolute equality of weight or *inertia* in the atoms or molecules of homogenous substances. Moreover in gases, liquids and chemically homogeneous solids we must conclude that under a uniform temperature, towards which there is a constant tendency, absolute symmetry of atomic arrangement and motion must obtain, since in every portion, of whatever form or dimensions, we find precisely the same specific gravity, that is, equal numbers of atoms in equal spaces. Still farther, when gases are brought into contact with different gases, liquids with liquids (except when immiscible), and even when certain liquids and solids are mixed, a diffusion or solution takes place and the two or more sub-

stances become blended into a most intimate association in which the atoms or molecules must be symmetrically arranged. Hence we may safely conclude, although in the absence of *a priori* mathematical demonstration, that there is a tendency among the atoms of homogeneous bodies to arrange themselves in symmetrical positions and to move in symmetrical orbits. Indeed dynamical equilibrium is hardly conceivable under other conditions.

We have already seen that in the gaseous condition, excess of momentum places the atoms in the relation to each other of virtual repulsion. They will therefore tend to assume positions at the greatest possible distances apart, each atom tending, by the laws of the dynamics, to preserve its own direction and plane of motion. At the same time, when confined within limited spaces, mutual gravitations should bring them periodically into contact or near proximity with each other. The paths by which the most direct approaches and yet the widest separations could be made would seem to be rectilinear ones, crossing each other at right angles in three directions, and dividing the space occupied into imaginary cubes. Along the edges of these cubes lines of atoms would pass in both directions, so that at the angles of the cubes simultaneous collisions or very near approaches of *six* atoms would continually succeed each other. Collisions of *pairs* of atoms moving in opposite directions would also occur all along the edge of the cubes. All these collisions or near approaches are accompanied by a previous augmentation and subsequently retardation of the velocities of the atoms. In general, as the average distance from each other is diminished, the average momentum by mutual gravitation is augmented, and with it the outward pressure of the gas.

Supposing the atoms to move in straight lines, the "law of Mariotte," namely, that at equal temperatures the volume occupied by the same quantity of gas is in inverse proportion to the pressure it exerts or sustains, is a direct consequence or corollary of this hypothesis. We have only to substitute *equal momenta* for "*equal temperatures*," and it evidently follows that the aggregate pressure must, in a given volume, be proportional to the number of atoms, or conversely, the

volume occupied by a given number of atoms must be inversely proportional to the pressure.

The variation from Mariotte's law which takes place under a pressure nearly sufficient to produce liquefaction, may be due to deviation from rectilinear motion, indicating a change towards the closed orbits of the liquid condition which will presently be considered.

The chemical principle that all gaseous atoms or molecules occupy equal volumes, is accounted for with equal simplicity as follows.

Equal temperature means equal *momenta* of the atoms. *Momenta* is weight multiplied by velocity. Suppose we have two containing vessels, similar in form and equal in capacity. Let one of them contain a certain number of atoms of oxygen moving with a certain velocity, and let the other contain an equal number of atoms of hydrogen, sixteen times lighter, and moving with sixteen times the velocity of the oxygen atoms. The momentum or temperature will then be precisely the same in each vessel. We have now only to prove that the pressure upon the similar sides of the two vessels will be equal and it evidently follows conversely that when the pressures, volumes and *momenta* are equal the number of atoms must be equal.

Since the atoms are equal in number and symmetrically arranged, it follows that upon every equal portion of the containing surfaces there must continually present themselves an equal number of atoms. Since each of these atoms has, in either containing vessel, the same momentum, the pressure upon each equal portion of the containing surfaces must be equal, hence the entire pressure upon the containing vessels will also be equal.

It must be remembered that the question of *pressure* is an entirely different one from that of *work*. The amount of work which the oxygen and hydrogen atoms are capable of doing is widely different as we shall presently see. If, for example, the gases in the two supposed vessels were only separated from each other by a thin membrane or septum through which they could gradually penetrate, the hydrogen having more *vis viva* would at first pass through more rapidly than the oxygen, and

the side containing the oxygen would be swelled to a larger bulk; the tendency to symmetrical distribution would at length assert itself however, and the two gases would be found mixed in equal quantities on each side of the septum.

When change of gaseous volume is produced by chemical union, we must suppose that the chemically combined atoms arrange themselves into systems, each of which, so far as heat motion or the mutual action of bodies to produce equilibrium of temperature is concerned, plays the part of a single atom.

In the hydrochloric acid type, for example, where the gaseous bulk remains unchanged, we infer that each atom plays its own role in heat operations. In the water type the single atom of oxygen plays one part, and the two of hydrogen another; in the ammonia type, the single atom of nitrogen plays one part, and the three of hydrogen another, and so on throughout all the infinite variety of chemical combinations. The volume of the compound gas, could we obtain the combination in a gaseous form, would accordingly determine into how many systems, each equivalent to a single atom in its heat functions, the combined atoms had aggregated themselves.

The development of heat by the compression of gases and its absorption by their expansion is another evident consequence of this hypothesis. By compression the atoms are brought under a more powerful gravitating influence, which increases the momentum, or, in other words, the temperature of the gas. In expansion exactly the reverse of this takes place; the atoms being withdrawn to a greater mean distance from each other are less reënforced by gravity, and their *momenta* or temperatures are reduced.

The remarkable law discovered by Dulong and Petit, that the specific heat of elementary bodies is inversely as their atomic weights, may be considered as direct evidence in support of the hypothesis that temperature is momentum. For if we admit this hypothesis, remembering that the measure of specific heat is not one of absolute associated force or momentum, but of *vis viva* or *work done* in increasing the velocity of the atoms, a measure of *space effects* produced or to be produced, it obviously follows that more of this kind of quantity will be required to produce the same momentum in a

light atom than in a heavy one, in the exact ratio of the greater velocity imparted, or in the inverse ratio of the weights. This is just what Dulong and Petit have, by careful experiment, established in regard to many substances, both simple and compound.

In tracing the transformations of matter from the gaseous to the liquid, and from the liquid to the solid condition, we cannot be guided by *a priori* or mathematical investigations, and can only suppose such changes to take place in the paths and motions of the atoms, as, while conforming to the general principles already stated, will most fully coincide with the actual facts in the results to be expected from the theory.

We may, for example, suppose that in passing from the vaporous or gaseous to the liquid condition, the momentum of the atoms falls below the potential of gravity, so that instead of moving in continuous directions, the lines of atoms break up into closed orbits which are long ellipses or loops in form. They may be *interlinked* at their vertices in one direction in each plane.

At the moment when this occurs some remarkable changes take place. The atoms drop down upon each other from the distances due to their previous direct motions and the virtual repulsion of their surplus momentum, to that which will give equilibrium to their new closed orbits. The entire distance thus passed through of course depends upon the pressure to which the gas or vapor was subjected before it became condensed to a liquid. Thus water is about seventeen hundred times more dense than its vapor when the latter is confined at a pressure of fifteen pounds to the square inch.

All the atoms in a single orbit probably have now a single direction of angular motion, and stretch around in comparatively close proximity to each other, somewhat as a meteoric ring may be supposed to pass around the sun.

A reënforcement of gravity is produced by this approach of the atoms, assisted perhaps by the interlinking of the orbits, which reënforcement is accompanied by an acceleration of motion and consequent increase of temperature, or development of "latent heat."

Some partial idea of the immense quantity of force associ-

ated with the atoms of a liquid, and their consequently enormous velocity may be obtained by considering what amount of velocity would be generated by the force which would be required to raise a quantity of water, for example, one degree in temperature. This velocity for water would equal that generated in a fall of seven hundred and seventy-two feet, namely, somewhat less than 7×32 or two hundred and twenty-four feet in a second. If now the temperature of the water above absolute zero is, say four hundred degrees, we have a velocity of $\sqrt{400} \times 224$ or four thousand four hundred and eighty feet in a second. It is not at all wonderful then, that water and other liquids resist compression, with such enormous force for any compression must still farther increase the velocity of the atoms and their consequent momentum and power to resist pressure. Each atom will be impelled against the compressing surface (or around its exterior atoms) with a velocity more than twice greater than that of the swiftest cannon ball.

In the liquid, as in the gaseous or vaporous condition, we find that the atoms or molecules are free to obey the impulses of any external force so long as it does not tend to reduce the amplitude of the periodical motion, or what is the same thing to compress the liquid into smaller bulk.

If we investigate the effect of a still farther diminution in the heat or velocity of atoms, we shall probably find that the long, elliptical, or loop-like orbits of liquids become gradually somewhat reduced in size and changed in form, so that at a certain period, a second or *lateral interlinking*, at the extremities of the conjugate axes or diameters, takes place, and a form of molecule is originated which gives the rigidity of structure found in solid bodies.

Doubtless the most direct path to positive knowledge in regard to the forms of solid molecules, is through the study of crystalline forms and forces. It is now generally understood that cohesive attraction in homogeneous substances is identical with the polar force of crystallization. A glance at the general features of crystallogeny, as now taught, will be likely, therefore, to render essential aid in the investigation.

Professor Dana, in the introduction to his great work on mineralogy, reduces all the various forms of crystals to

primary ellipsoids, which he supposes to be held together by a powerful attractive force which suddenly comes into existence at the instant when crystallization takes place, at the extremities of conjugate axes of the ellipsoids.

While this hypothesis is amply sufficient to account for all the various *forms* of crystals, which it does in a simple and beautiful manner, the *source* of the polar force, by which the previously liquid molecules are compacted into the hard unyielding crystal, is left as much in the dark as before, and such questions as the following naturally occur to the student. What is the nature and constitution of this ellipsoid? Is it solid, hard, elastic, etc.? Why does the polar force remain dormant in the liquid, and suddenly start into existence at the instant of solidification? Why is it confined to six points on the surface of the ellipsoid? To these inquiries, the most diligent questioners of nature seem as yet to have received no satisfactory response.

In answer to such of them as have not already been answered in this paper I venture to make the following suggestions.

Upon the surface of the primary ellipsoid of Wollaston and Dana, suppose three ellipses to be described by the intersection of bisecting planes parallel with the sides of the circumscribing prism and to the planes of crystallization. These three ellipses will intersect each other at the six poles of crystalline attraction. Removing the ellipsoid, we will retain only the imaginary ellipses, which we will suppose to be replaced by the rings of atoms already described.

Solid molecules as before described are each made up of at least three rings, revolving with enormous rapidity in different planes, and intersecting each other at the six crystalline poles, each ring interlinking at four of these intersections with the corresponding ring of the adjacent molecule, thus calling into existence at the six poles that powerful attraction which was to be accounted for. Moreover the polar property of this attraction, which has heretofore effectually baffled its investigators, is clearly accounted for, being due to the *direction* of the planes of atomic motion, and the tendency to preserve these planes, and continue them from molecule to molecule.

To account for the ellipsoidal forms of the molecules, we have only to bear in mind that in a body of uniform temperature the mean *momenta* of the atoms are equal. If, therefore, the rings forming a molecule are unequal in weight it follows that the heavier rings will move in correspondingly shorter orbits.

In the case of chemical compounds, these rings as already suggested, might be made up of secondary systems analogous to those of planets and satellites in the solar system. These systems would most likely be so made up as to introduce no exaggerated and unstable disproportion into the dimensions of the elliptical rings forming the molecules.

Supposing each of these rings to be made up of similarly com bined systems, the resulting form of molecule would be that of the sphere, the primary form of the monometric system of crystals. It is easy to see how all the other primary forms of crystals may be derived, by introducing rings of various lengths into the primary molecules.

It seems not improbable that, by carefully studying the modes of arrangement into systems and rings, the calculation of crystalline forms may be reduced to a problem in dynamics.

The limits of this paper will admit only a hasty glance at the hypothesis of the *universal ether*, now generally supposed to occupy celestial spaces, and to constitute the medium by which the light and heat of the heavenly bodies is transmitted with such wonderful velocity. If we admit the existence of such an ether, there seems to be no reason why it should be considered so different in its nature from other matter as to require a different definition or different laws of association with force. It is only necessary to remember that its atoms are relatively almost infinitesimal when compared with ordinary chemical atoms, since they have, so far, effectually eluded all attempts to weigh them, either by their retarding effects upon celestial motions, or by their incorporation into any chemical compounds. Hence in accordance with the dynamical principles already stated, they must move with an almost infinite velocity when compared with that of even the lightest ordinary known atoms, as, for example, those of hydrogen. This supposition seems to accord with the almost inconceivably rapid transmis-

sion of light and heat. When we have accurately determined the temperature of the celestial spaces and other necessary data, we may calculate the chemical equivalents of the etherial atoms by dividing this temperature by their velocity. Thus, if we estimate the velocity of a hydrogen atom, at a temperature of four hundred degrees above absolute zero, to be three miles per second, and the velocity of the etherial atoms, at one hundred and ninety-six degrees above absolute zero, to be one hundred and ninety thousand miles per second, we obtain the velocity of the latter at four hundred degrees by the following proportion, $\sqrt{196} : \sqrt{400} :: 190000 : 270000$ nearly, hence the chemical equivalent of the etherial atom, reckoning hydrogen at unity, is $\frac{3}{270000}$ or .000001 nearly.

In settlement therefore of the long controversy between the undulatory and corpuscular theories of light it may be found that both are partly right and partly wrong. That is, the optical effect is not produced by a direct impact of particles as supposed by Newton, but by transmitted disturbances of the dynamical equilibrium, in other words, by *vibrations*, while on the other hand the rapidity of the transmission is due to the proper motions of the atoms.

With this outline sketch, which has already occupied too much of your time, I leave the subject for the present. I cannot doubt that the application of this hypothesis to chemical, optical, and electrical science, will furnish a clue to some of their hitherto unsolved mysteries, and especially in the elucidation of that great principle of universal polarity or simultaneous exhibition of equal and opposite forces, for which no adequate cause has yet been produced. Ampere's beautiful theory of the identity of electricity and magnetism, for example, seems to be a confirmation of the hypothesis I have advanced. Indeed, the figures with which he illustrates the currents of electricity in magnets, seem to be drawn almost on purpose to indicate such atomic motions as I have been describing.

26. The Volcanic Tide Belt and the World's Flood-Gates. By Theodore C. Hilgard, M. D., St. Louis, Mo.

It is now the time to call attention to the fact that all large open sea-expanses (exclusive of all the tideless inland seas, such as the Baltic, the Mediterranean, Caspian, and our Lakes) are actually underlaid by cold strata, at a rapid rate deprived of heat, by the convection, at large, of polar undercurrents, often a great deal below the freezing point of fresh-water or ice, and by miles of hydrostatic height—and thus are notoriously of a much lower temperature than the adjacent continents at corresponding depths. Therefore, the large oceanic depths being more uniform, and at the least by as much thicker than the former, as the height of snow-line plateaux of continents is above the sea, and being accordingly of a specific density thereto corresponding, it follows that the crust of rock underlying the oceanic hemisphere, and the Arctic Basin, likewise, must represent a submarine impending inverted continent whereof the refrigerated, semirigid, basal mass is sealing down, as it were, by the semifused volcanic substances, our central bottomless pit of unquenched fires, with a liquid centre, and only incarcerated from above. Now the rigid crust under the sea is far more ponderous than that of the low continental expanses at corresponding depths, and often probably protrudes a jutting broadside, downward into the volcanic interior.

Nor is the geographical relation of volcanic action as yet fully appreciated in geophysical science.

The "continental" hemisphere, actually centering near the prophetic queen city of the "orbis terrarum" (or terraneous semiglobe) is a dome, or terrestrial cupola, the centre whereof as a pantheon's* "sky-light" is perforated by Mounts Etna, Stromboli and Vesuvius, etc., and is divided from a preponderatingly "aquatic" or "oceanic" one by an imaginary dividing-line such as, *e.g.*, claimed by Humboldt and other geographers; but which, if laid only ten degrees farther north, at its northern apex, in the Pacific Ocean, while actually containing as much oceanic expanse, when coinciding with the Aleu-

* or pandemonium's?

tian Archipelago will thus at once be found identical with the *volcanic belt of the Globe* as a "greatest circle," almost undeviating from a girth cutting the equator at Sumatra (or in the explosive East Indian volcanic Archipelagoes) and likewise antipodally explosive in the equatorial precinct of the late disastrous earthquake; the famous province of Quito, with the Cayambe at latitude 0°, and in the immediate neighborhood of the clustering cones of Chimborazo, Cotopaxi, Antisana, Pichincha* and the volcano of Pasto farther north.

It must be recollected that this volcanic girdle forms an unbroken continuity, from the East Indies, or the Sunda Archipelago, through the Philippines, Loo-choo, the Japanese and the Kuriles Archipelagoes, and all the length of Kamtschatka, tending in a north-easterly direction, where the famous Aleutian Archipelago joins at right angles, and, together with the Peninsula of Alaska, forms nearly a quadrant of a circle around a point, almost within Behring Strait, and exactly on the Arctic circle, 23½° south of the present North Pole; through Alaska, British Columbia, Oregon, and almost striking the South Pass — that original crest of head-waters diverging into four important rivers, the Columbia, the Colorado, the Missouri and the Platte rivers — on through California, Mexico, Quito, Peru, Bolivia and Chili, this eruptive crack of our "earthen" Globe; switching off rather to the right, to Cape Horn, is thence dotted out by a rectangularly incident linear Archipelago composed of the Falkland Islands, the Aurora Islands, New Georgia, Brown's Islands and Sandwich "Land" (not "Islands"), whence, after a short interruption — by only one-fifth of a girth — the line, swerving to the left, subsumes Madagascar, the so-called Mascarenes, the Seychelles, the volcanic straits of Babel Mandeb, the Laccadives and Maldives, rejoining the line of direction only at Sumatra, rather along the outlines of all the adjacent continents.

Although only an approximative girth of our world, it emphatically separates the aqueous hemisphere from the "terrestrial," so-called one, by a succession of eruptive fissures, almost invariably following the *crude configurations of continents always near the coasts.*

*Pichincha alone stands as the apex of a *crest*, the rest are "cones."

On the Asiatic side they follow the coast-line far out at sea. On the American side they follow the coast rather closely in the land. The West Indies, however, are predicable as belonging to the great equatorial explosive intersection-ground, and are a probably "recent demersion" with the late elevation of Quito for a paragon. In either case they stand eastward of the border of the Pacific adjoining.

It is evident, as Professor Perrey of Dijon has repeatedly mooted, that a Plutonic semiliquid or liquid molten mass must obey the Newtonian tide-agencies more promptly than does the sea; the latter being only a few miles in depth, against a radius of four thousand miles of unaccounted, but partially liquid, nature. Of course, then "the planetary elongation" wrought by the computed gravitative velocitarian difference between our Globe's centre and its zenith, as well as nadir, and that towards and under the moon amounting to about five feet, would mainly occur in the semiliquid or fused central mass. The confining crust for continents is computed to have about twenty-five miles thickness; but it must in high seas — of several miles depth and of very low profundal temperatures — be estimated a good deal more rigid, resistant and uniform, and at the same time projecting below the terraneous crust by the entire depth of the ocean, and to which must be added the excess of thickness by the incident rigor, or huge abstraction of heat by contact with, and convection of, gelid submarine torrents of immense thickness or height as propelled from either pole towards equatorial coasts, and found under-running and partly opposing, partly favoring, the circular sea-currents as they move toward — and then return from — high latitudes: and whereby the coasts of South America receive from either pole a cold undercurrent, naturally conveyed through cortical channels, depressed by their own specific weight being increased by cold temperature, and thus likewise agglomerative of more (congelative) "basaltic" masses, *thereby sinking all cold channels and cold sea-grounds below the level of the continental crusts:* and it is *this* feature which at once explains and subsumes all the phenomena intimated in the heading. All the islands of the Pacific have only a slight tide, whereas there the hydrographic features would be most favorable to the for-

mation of the superficial oceanic-sheet tide, which at the best could amount to only a fraction of an inch; whereas a uniformly and hydrostatically buoyant interior would heave up throughout (causing only a change in curve and in star-times). Besides, the fact shows all the tides to have the strongest power near the shores, and to be travelling waves, as notoriously admitted by all experts, with a velocity from east to west of at most three hundred to six hundred miles an hour; whereas the planetary rotation and coincident phenomena at the equator travel at the (rotatorial) velocity of one thousand miles an hour; hence the tide cannot be transmitted as a travelling wave! Whewell's well-reputed tide-chart in fact shows the *ebb to be directly under the culminating moon* at the five controllable points at the coasts, *near the equator.*

At Cape Coast Castle (longitude of Greenwich, with culminating moon at midnight), we find No. VI, which means tide at 6 o'clock, A. M.; hence, logically, ebb at midnight — or noon! Under the culminating moon thirty degrees or two hours farther west, at the coast of Brazil, we find VIII, which means ebb at 2, A. M. At a quadrant's distance we find the moon over the Gallopagos Islands, and full ebb from Lima to Guayaquil; and antipodally to Cape Coast Castle, we find likewise highest ebb at the Gilbert's Archipelago; and antipodally to Quito a full ebb at Sumatra, Ceylon, and Cape Comorin, all under the culminating moon! Now this is the true event and explains itself fully on the Newtonian premises of planetary elongative stress, but only under this condition that, *the rigid crust resisting*, the main stress results as a local *sublevatory shock or ebb* at the lines of least resistance, the warm and lowland coasts, causing a wave to roll off, toward the sea, which is not so pliant in its undercrust, and returning when the land is sunk, as the local *tide-roll!*

This fully explains all the tide-wave reverberations and retardations so aptly shown by a glance at Whewell's chart: both sides of South America, for example; while still the exciting cause, *i.e.* the sublevatory ebb-shock, unimpeachably performs its "underground railroad" motion after the well-known rhythm of a tide — or ebb — twice in twenty-five hours, as required by the positive fact of observation.

A system of five synchronous tides, and as exhibited by Whewell's Chart, thus becomes at once explicable, although one of them is at least a very dubious and at all events a supernumerary one on the supposition of a tide-roll itself performing the transit. It is the dodecad of intercalary sea-rolls inscribed between Adelaide and the Cape, whereas the text requires, instead of twelve hours, but one single hour's difference. A triad of synchronous wave-rolls would produce a tide three times in twenty-five, or about once every eight hours; which is contrary to the established rule.

As we can only rule a tide or ebb where there is land, the high-sea rolls escaping observation, all facts agree with the assumption of a subterraneous upheaval or ebb travelling once in twenty-five hours around the Globe. At the mouth of the Amazon we find a "bore," or rocker (hence "Pororocca") at Para, just before high tide. At ebb the moon is culminating there—*fide* Whewell. As the roll of the tide, cast off north-eastward, returns south-westward, and while the moon is already deeply sunk behind the Pacific Ocean, both the local tide and the slow African roll coming in by this time across the narrow Atlantic, this "bore" is produced. Again, the highest tide at the coast of Lima, etc., is when the action of the moon reverberates from the deep, cold, jutting bottom of the Pacific against the hot and thin coast, backed in the rear by the "heart of the Andes," so that the regurgitation swells the ebb to its highest. So is the high tide of all the Pacific shores due to the translatory under-rush recoiling from the Pacific bed as high ebb, and the tide afterwards rolling in broadside from the Pacific. The "bore" of the Hoogly or Ganges is evidently produced by the recoiling wave from the equatorial parts being narrowed in within the triangular Gulf of Bengal; and the Himalaya Mountains (close in the rear of the Ganges Deltas), strongly opposing the upheaval, the resultant relative depression in the rear actually diminishes the head or fall of the entire river-length sloping along the mountain-chain; and so the constricted northward roll finds a diminished head and not a backing up of rear waters, and hence the bore results.

As to the Maelstrom, it receives the gulf-current's Arctic outrunner, through the only channel half a mile deep, left be-

tween the Shetland and the Faroe Islands, in that grand submarine shoal or dyke of one-fourth of a mile in depth all the way from Scotland to Iceland, and all included in that tract which, according to Humboldt, is in a slowly rising condition. Now, every check of circulation of tropical waters towards polar ones, and in the case of the Arctic Ocean, every interception of the Asiatic or Atlantic gulf-currents may slowly eventuate in a perfect glaciation of the Arctic Sea and its adjacent countries!

Occasionally the diurnal sublunar-coast-impact from beneath rupturing the coast under ground may thus give access to either marine or (heated) artesian waters, often of more than explosive temperatures—as manifest in the Geyser-phenomena, and sufficient for themselves to account for all "metamorphic" changes, by the incalculable dissolving (chemical) power of waters overheated (under pressure of their own column, or either, of confining rocky strata)—and cause them to force apart the fissured heated rock, down to the very lava not itself necessarily acting under any explosive stress; by which contact—as by a fused metallic mass exploding water-pipes—the explosive shock, mainly propagated by the artesian sheets, as an earthquake, it is already presumed by some former authors, can arise under the agitating influence of the spontaneous impact of Plutonic masses, gravitant towards the moon.

It is by the evident oscillatory and reverberative bis-diurnal rising and falling of coasts and continents—vide South America—that, by clashing and recoiling, not only progressive wave-shocks are retarded, as per map; but that by their meeting and redoubled roll: back upon an already sinking border, those heavy tides eventuate both on the eastern borders of the Atlantic, from the Maelstrom clear down—outside of the European inland-seas and ensconcements—to the Cape of Good Hope; and on the eastern border of the Pacific, from Alaska to Patagonia: and the littoral shock that raised the coast so as to ascend, as an "ebb," causes the same wave which returns from the high-seas to recoil upon a border already dragged downward by the heavy plunging (cold) eastern submarine "plateau renversé" of sea-bottom.

The very shock experienced at the coast is a proof of the

practical, partial impliancy of the submarine crust, by showing a deflection of ascending thrust towards the sides! It hence follows that the "planetary elongation" (with its utmost claims to about six feet of difference) has been somewhat checked, having resulted in a local derangement or divergence between land and sea, as ebb and tide, and that is a never-to-be-forgotten source of unequal horizon and unequal "transits" or "star-times" thence infallibly resulting!

Now as to the sea-water, with an impact at six feet differential velocity perpendicularly towards the moon, etc., in advance of centre: it cannot rise much in any place directly under the luminary; it simply diminishes its weight, it is true, and only by collateral influx of masses at an octant (where the impact favors a tangential rush, and that need not leave the crust, underneath, in following their impulse) it is somewhat raised, though little; whereas the actual, although reduced, circulatory gyration, required, of at best six feet diameter, cannot be executed by an entire sea underlaid by a rigid crust; and therefore the motion that, in the Plutonic mass, pivots—and is mutually poised—on the centre, in the case of surface sheets above a crust, is sidewise deflected towards the poles as the elongation of that centre; and it is probably an entire sea-depth of several miles, progressively impinging at a circulatory motion on an almost unyielding platform, that the equatorial east-west current, and actual surface-rush, results as an excuse and partial attempt at an uninterrupted circular current sweeping the equator (such as it no doubt has once existed), and which, collecting all (hot) surface waters, now sends them flirting sidewise, towards the pole as an excuse for a centre, thus producing that returning west-east sense of drift that carries the cold polar undercurrents' exchange-waters south-*east* from the Arctic region—instead of south-west, as tend the trade winds!—and north-east from the South Pole to Peru!

Another consequence of the inequality of sea-grounds is manifest in the evident relation of the Asiatic pelagic chain of volcanic action parallel to its warm gulf-current. It is there that the crust, by its high temperature and consequent tenuity, is most liable to "breaks" or volcanic explosions under the

contracting power of submarine cold in the ancient Pacific, aided by the attractive agency of the moon, most powerful in its perigees and conjunctions with the sun, as argued by Professor Perrey, and so disastrously confirmed by the late convulsions at Hawaii, occurring mainly the day preceding the perigee April 3, 1868, and in South America during the weeks of the perigee Aug. 17, 1868, commencing after the date of our adjournment from Chicago (August 12), where the presaging assertion was uttered, before the section, that "the coast of South America, the hottest and narrowest strip between the most stupendous mountain-elevation skyward, and the most precipitous profundal trough-lines or cold-water crests abysmally reflected, has never been inhabitable (witness the unobserved entrance of the conquistadores under Pizarro into Peru), and will never come to rest, being riven and convulsed under the moon, as long as its Andes lie afhward the equator;" unless all the most important geographical features of the Globe be changed, and whereof I will here offer a short synopsis.

In the first place, aside of the ancient Altai or Cobi plateau in Asia, we find a north-east range of uninterrupted main-line of mountain-elevation, commencing at the terminus of the Himalayas, at the Brahmapootra river: the continuous rear-range of Assam, China, Mantchooria and Siberia, up to Behring Strait.

This range then is parallel and "twin" (so to say) with the corresponding east Asiatic "volcanic fissure" off-shore, cleaving the main from the explosive Sunda Archipelago up to near Behring Strait, as above detailed.

The Japanese gulf-current is parallel to it, parallel to the whole explosive libration of that coast itself engendered by warm currents near the old Asiatic restricted plateau of the Cobi, once the fabulous "Paradise Lost" of the Chinese!

Is it not probable that the advent of uncommonly cold waters or an uncommon perigee of the moon caused such disruptions along the lines of least resistance? The same applies to the Alleghany and Scandinavian mountains. Opposite Asia, along the same ocean on its eastern borders, in America we find a direct, uninterrupted chain of stupendous mountains

and volcanoes directly along the "cold, deep water" edge, and from Alaska to Patagonia. Alongside of its Andes, the ground of all South America is so much depressed, so little elevated above the sea, that in the wet season an inland continuity of water is established between the low river-beds of the Orinoco, Amazon and La Plata, draining an extent as vast as that from our northern Arctic border to the Gulf of Mexico.

A very slight "dip" would cause all South America to disappear from the face of the Globe, leaving the chain of the Andes to play a part like that of the submarine Asiatic volcanic (N. E.) crest.

It is from Behring Sea and the Aleutian Archipelago that the North furnishes the submarine exchange waters which are deepening the Pacific by a cold undercurrent all along the shores of North America from Alaska to Quito.

This abrupt deep channel is overrun with the circulatory Japanese gulf-current, deflected eastward by the interposition of the submarine volcanic Aleutian chain and the cold northerly return-waters drifting in between it and the shores.

THE DEEPENING OF OCEANS

withdraws the waters from the surface, sinking their coral-reefs so often quoted in proof of the ancient shallowness of the open oceans. The same sinking of sea-bottoms also buoys up or denudes, relatively and positively, all continents, and inland seas such as the Caspian, Baltic, Mediterranean, and Lakes; the coast of Norway and Sahara deserts, no less than the saline pampas of La Plata; and in all continents we find huge areas rather suddenly uplifted towards the snow-line; phenomena gradually arriving together with the submergence of the ocean's archipelagoes, and demonstrative of polar undercurrents having been formed probably by the deflection of the original equatorial ring-current.

If the Aleutian chain did not oppose the Japanese gulf-current, then as it might be, the Arctic Sea would fully receive, two rapid, heated currents, conjointly to be drained by Baffin Strait, their only vent left.

The Arctic Sea would then become a warm or temperate

one, and by the ensuing dissolution of heavy bottoms, the open oceans would become warmer, and hence shallower.

The islands would become higher, larger, and rather uniformly temperate, and the continents would relatively sink down towards the Plutonic mass, become smaller, lower, and from underneath be on an average more uniformly heated. Also the seas would be less heated above, less frigid below, than at present!

If once the Aleutian Archipelago did arise and thus first deflect the Japanese Gulf, as an ancient hot-water current partly tributary to our Arctic Basin, then perennial frosts must have ensued at the North Pole, and the whole Arctic Sea been sunk, together with all open oceans, by acquiring a denser, colder, heavier under-crust, denuding northerly all the coasts of Siberia and Hudsonia, as it is now revealing that frozen, once submarine "drift of pachydermal carrion," then by a rapidly coursing gulf-circulation conveyed intact to northern coasts, where at Kotzebue Sound, above Behring Strait, the "Herald Expedition" (in search of Sir John Franklin) discovered a denuded, once submarine, ice-plateau of from thirty to six hundred feet height above the modern sea, and several feet in thickness, overlaid by a drift of pachydermal bones and mould of animal origin not yet fully decomposed, as it were; the perpendicular cliffs still jutting down under water to the bottom of a sea on account of its saline ingredients remaining liquid, while too cold to dissolve ice once through freezing separated from the brine.

A slight depression of oceans — as by the lowering of the general sea-bottoms often referred to — would cut off all other shallower gulf-currents, such as the probable ancient Baltic and Mississipean ones, and by also laying dry an Arctic exit (now Mackenzie river and Undine Region) first grate down the slue with drift-ice, and by a final preclusion add to the Arctic rigor thus suddenly supervening.

If at one period the Aleutian chain or the narrows of Behring Strait so far projected over the sea as to preclude the entrance of Pacific waters at that point, it would not only have left the Pacific to circulate and recoil back upon itself — a shallow heated sea deprived of an Arctic undercurrent — but

then the excessive cold, produced by a sluggish Arctic circulation only under stress of the Atlantic gulf-current, and then alone issuing back through Baffin Bay with small force; would not only have stemmed and deflected that current at its very entrance, so as to make it recoil upon its own bosom, thus causing a profound, but heated ocean, violently circulating, in its temperate latitudes, with but a scanty undercurrent, in the Atlantic likewise; but it might finally *freeze up Baffin Strait*, and cause the northern basin to stand as an *immovable pack of ice* overrunning all northern border lands with an accumulation of glacial masses, enough to cause, on a smaller scale, that eminently historical refrigeration of all northern borders, which avowedly caused the post-christian *migration of nations* both in the old (Mongols and Swedes) and in the new (Caribs and North Mexicans) world; but the *rising of the Iceland-shoal* would also fully account for all the features of a "glacial epoch" which, as Professor Tyndall so aptly suggests, requires as much a hot climate on the one, as a congelative, immovable, cold one on the other side!

And it is this explanation I here offer, of a temperate universal middle zone of our Globe, adjacent to a pitiless cold north pole, and productive of the incredible extension of glacial formation in the first place, by a *non-deflected circular equatorial current* (somewhat like the ring of Saturn); secondly, on a smaller scale, by a partial preclusion of Pacific and Atlantic influx—through the Aleutian Islands and the Iceland-shoal—on the one hand.

And as to the disappearance of that period, and wasting of its glaciers, I offer this explanation, that the *breaking-up of the equatorial current, and the opening of Behring Strait*, or the submersion of a more projecting Aleutian barrier, caused the seas to sink by a cold exchange undercurrent, and the glacial accumulations to melt away by the advent of warm currents; and if that barrier continues to sink, the Asiatic current might yet more fully enter by Behring Strait, and leaving by Baffin Strait, in quick exchange still cause all the seas to rechafe at bottom and rise, and our continental expanses to once more sink toward the Plutonic fires; their Neptunic strata, their swamps and peat-bogs once more to transmit to following

generations the phenomena of "metamorphic" rock, of "stone-coal" for peat, and the petrified impressions of the world we live in at this present period!

In conclusion let me remark, that what on broad evidence we can at large construe, on granted principles and given causes, is far more reliable than what we have to explain by hypothesis alone, and on often quite irrelevant "exact measurements" where the very minuteness or sensuous "exactitude" required, is often the root of latent error; whereas, when by *logical* exactitude in our synthetic applications of the known; by a liberal exegetic

GENETIC UNDERSTANDING:

of causes and effects; of logical premises and inferences; and (in the organic world) by the study of developments in their commencement and end: when by an intelligent appreciation of the normal we can substantiate the distinctive or "abnormal," and science on its evidence instead of on *credence:* then, by understanding the motor agencies of the present, we may explain the past and control the future; above all, it will point the way whither to direct our suggestively intuitive energies, our labors and observations, both plausibly and profitably.

EQUILIBRIUM AS EQUAL VELOCITIES.

All the foregoing phenomena, as well as all barometric ones, and both the atmospheric and hydrothermic exchanges from the Pole to the Equator, and *vice versa*, all being based on the condition of a *horizon of equilibrium* I would here briefly submit a few considerations.

If a solid globe of homogeneous attractive power is set to rotate, then the resultant of forces will cause the pendulum as well as any other mass to "vary," both as to velocity and the direction of each resultant: which resultant effect withal constitutes "weight" proper; namely, the compound effect of the theoretic attraction of masses and of the sublevatory effect ("flirt") of tangential inertia, which sublevatory, antagonistic resistance to the "attractive" factor, I would here beg leave to plainly designate as "rotatorial dip."

If under such assumption of "true sphericity" the pendulum must vary (for a planet, of the bulk and rotation as those of ours, computed at the proportion of about $\frac{1}{288}$), if *not* aplanated: how then should it be consistent to state, that (the observed) "variation" (of pendulum) should argue aplanation? If really so observed, would it not argue sphericity? This is the question.

If, however, we consider a planet adaptable according to hydrostatic necessities, not only as by an external liquidity (as of oceans) but also by a probable profundal liquidity of Plutonic, fused or pliant, hydrostatically "terrigneous" masses; if, as we actually find it, the presence of the ocean level both at the poles and the equator, and also the uniform barometric height at all sea-borders argue a true horizon of equilibrium, how is it that the pendulum should be said to vary, and that "variation" to be *corroborative* of aplanation; whereas, aplanation, such as geodesically measured, argues a form of *equal* hydrostatic surface velocities at right angles to tangents, —or, in the sense of the "normals"—thereby producing a uniform, connected, hydrostatic surface, and which otherwise could not obtain! Why or how is it, that the argued *compensatory elongation of the Globe* should not work as a "compensation on surface particles?"

If the variation ought to obtain in case of sphericity, then it ought not to obtain in cases of equilibristic aplanation. If such variation proves sphericity, the same one (approximately so) cannot prove aplanation.

In all popular and established school-books, and Astronomies, I find at certain junctures the factor of "attraction" substituted by "gravity," which grammatically means and is weight; technically, however, is meant for only one of the distinctive factors of weights whatever; namely, Newtonian mass-"attraction." This is a dangerous usage of an ambiguous expression, for immediately follows, *e.g.*, in Herschel's "Outlines of Astronomy" (¶ 231) a commutation or substitution like this, "there is actually observed to exist a difference in the gravity or downward tendency of one and the same body," etc. Here "gravity" is urged in its grammatical sense, instead of the technical distinction, and at that in order to

initiate the (true or apparent?) contradiction of a compensatory or "equilibristic" form *not* producing compensation or "equilibrium!" This seems not to be consistent. Or, can it be said to be consistent?

It ought to be recollected, that when the question is of hydrostatic pressure, the weight cannot come into consideration neither as "specific" weight, nor as "absolute" weight, for neither are solid weights meant nor are hydrostatic weights absolute, but (like any other weight whatever) entirely dependent on the extraneous masses! Nor are these weights detached masses! We speak of a "weight" of one ton or two tons—this comes not into consideration; nor either "specific" weight. All that can come into consideration in hydrostatic phenomena, is the resultant *velocity* and that in a compound sense of *direction* resulting from two distinct factors, and which resultant or perpendicular surface velocity in a case of elliptic aplanation is not mutually reflected only by the absolute height of the column (as, *e. g.*, in the case of close proximity to a uniform level or horizon); but each column has its own index of velocity; namely, in the short or polar axis pure attractive velocity, and in the sub-equatorial dimensions both the "attractive" velocity and its sense of direction modified and diminished by the rotatorial dip velocity; and if, as it is agreed, and must be, the "force" of each channel is equal—as required for equilibrium or "equal impinging velocities"—it follows that as a higher equatorial column, when diminished by flirt still exercises equal power with the (inscribed) polar radius; it must be that each measure of the longer column is operated upon by a lesser compound power than the same geocentric measure of the polar column is operated upon; but that by the urging of the polar column down to the centre, so much "head" has been bulged up into the equatorial column as to produce an increased amount of attraction in that sense; that, short of $\frac{1}{580}$ relates *conversely to radii*, and that, although diminished by the incident tangential loss by the dip or flirt, yet leaves a resultant velocity toward the surface (not one of masses but of a hydrostatic thread), equal to the velocitarian impact at the apex of polar column. From a mere arithmetical point of view, is it not plain that although each

particle or measure has been rendered lighter in the equatorial sense, still the *height* of that column or its "corresponding points" (fractions) are raised to such an extent as to *attract more powerfully as a whole?* for being able to just balance the loss arriving through flirt? so that, *e.g.*, supposing the liquid equatorial column to be thus far heightened as to draw, by mere attraction, any falling body seventeen feet per second, and that second's dip amounting to one foot, this would still leave the compound resultant velocity sixteen feet per second towards the surface!

Nor can a *surface be maintained* except under the identical impact at velocities collaterally exercised each in its own resultant, and producing rectangularly a complete "surface" of virtually "equal stress," but of different distances from the centre.

Therefore, in a "figure of equilibrium," the rectangularly impinging surface-velocities in order to agree with equilibrium must be *equal;* and hence, in a true figure of equilibrium, the *pendulum*, the lead, a stone flung, ought to "bear" equally all over the surface!

Still, after having once fully "compensated" between the factors of pure attraction and that of rotatorial fling, La Place, Herschel, and others, directly proceed to deduct the flirt *once more* from the uniform compound result (namely, equal stress or "weight): and this only in order to usher in the received "pendular variation."

How is it then that in a form of compensatory velocities, where the spherical form is denied, the compensation should not appear achieved in the pendulum, likewise?

I think the question opens. What the *pendular data*, by experiment, actually were? I have no records to find out. All I can find are tables of "reduced" pendular results, by which, if conducted after the form which Arago in his Popular Astronomy, book 23, chapter 12 (but perhaps too loosely only) gives: by referring positive results, originally equal enough, instead of "to the sea-level" (as required and also averred) actually to *an absolute distance from centre* the actual collateral equality would have been forcibly retranslated into the diversified rates of radial planetary compensation, and a true com-

pound equal result been made to show only differences of *radii* supplied, but not of strokes, and but just themselves required to produce that actual "equality" of pendular velocity!

I here recommend the whole idea, as popularly received, together with the (true or apparent) incongruities involved, to the renewed attention of philosophers, in order at least to find a form of linguistic communication to us "outsiders" to remove such (true or apparent) inconsistencies, which, necessarily, only contribute to confuse and deter the student of geophysical phenomena.

B. NATURAL HISTORY.

1. On the Evidences of the Antiquity of Man in the United States. By Colonel Charles Whittlesey, of Cleveland, Ohio.

Within the past five years remarkable developments have taken place in Europe connected with the antiquity of man; not less then a dozen books have been published upon the recent discoveries, most of which are written by men of learning, and show the results of their personal observations. The number of the relics of man thus brought to our notice is surprising. In the delta where the impetuous Tiniere discharges into the Lake of Geneva at Villeneuve, in Switzerland, Mr. Morlot has traced the successive occupants of that country from the Christian era back to the age of Stone.

The savans of Switzerland have discovered and explored large numbers of the habitations of a departed race, who built their domicils upon piles, in the shallow water of the lake near the shore, and who were also of the era of bronze, and stone implements. Retiring still farther from the historical epoch, the French and Belgian geologists and ethnologists have found, in the numerous bone caverns of those countries, utensils fabricated by men, and bones of the human race, under circumstances which have led these investigators to date their existence nearly or quite to the glacial period. These announcements by the continental explorers were received by the English geologists with astonishment, and in most cases with incredulity.

In 1863 the patience of the English investigators was nearly exhausted, by reports made by M. Boucher de Perthes, at Abbeville, on the banks of the Somme, that he had exhumed a human jaw from the superficial materials of that region. He had in 1837 and 1844 announced the discovery in the same beds at a depth of ten to twelve feet, of numerous wrought

implements in flint, made from the silicious nodules of the chalk formation on which the Abbeville drift materials rest. During a quarter of a century these reports were generally discredited. M. Boucher de Perthes was not a man of science, but a gentleman of wealth and leisure, of high literary acquirements, with much French enthusiasm, and he had a theory to support. His brilliant general learning rather injured than improved his authority among those who pursue the exact sciences. He adhered to the doctrine that the flood of Noah extended over the earth, and that the knives and arrowheads in the drift of the Somme were the remains of generations which were destroyed by it. To find evidence in support of this belief, he pursued his investigations of the gravel beds around Abbeville, until one generation has passed away. His proofs were thus far only sufficient to convince a few personal friends of his own nation. His own life had passed the usual limit of human existence, yet the ardor of youth was in his case coupled with the perseverance of age.

A number of other fortunate circumstances combined to render his researches more thorough than any heretofore carried on in the drift materials. There is a canal in the valley of the Somme, which in one place gave a fresh section sixty feet in height. The railway from Bologna to Paris passes through Abbeville, and up the Somme.

For many years the common roads of Picardy have been made and repaired with the flint gravel of the drift beds, which was procured from numberless pits along the banks of the Somme.

The French government has built a large permanent fort near the residence of M. Boucher de Perthes, at Abbeville, in the ditches of which the same beds are exposed. M. de Perthes had for many years engaged the workmen upon all these improvements to search in the gravel for flint arrowheads, hatchets and splinters, by paying them a small sum for what they should find. He had also the good will of the Emperor, and consequently of the superintendents of the works. He spent much of his own time with the laborers in the pits, and had friends who did the same. His faith in the existence of human bones in the same beds where the flint implements were

found, never flagged, because he considered that he had himself seen decayed fragments of them; not, however, perfect enough to announce to the world.

On the 28th of March, 1863, the great object of a long life was accomplished by the discovery in a pit at the wind-mill of Quignon, about seventy-five feet above the tidewater at Abbeville, of one-half of the lower human jaw, near which were some human teeth. During the year which followed, several other fragments of human bones were found in a similar situation, in a layer of gravel near the overlying chalk, at depths of ten to twelve feet. These were in close connection with the flint implements, but the human remains were so small, and with the exception of the teeth so much decayed, that the workmen very seldom noticed them. They had, moreover, a superstitious dread of human bones found in a position so mysterious, and therefore rather avoided, than sought them. Most of these remains were discovered by scientific collectors, who broke open with their hands the lumps of clay and sand of which they formed the nucleus, which the workmen had thrown into the spoil banks. It is evident that nothing short of the presentiment, the faith, and the enthusiasm of M. B. de Perthes, making the most of the most favorable circumstances, would have brought these relics to light. Without all these happy coincidences the discovery might not have been made during another century. His disclosures led to close investigations along the valley of the Somme, and the bluffs which border upon it, up the stream to and beyond Amiens, a distance of about thirty miles, the formations being everywhere the same. Hundreds of flint implements have been exhumed from the same gravel beds, on the French side of the channel, and on the English side, in formations precisely the same; amounting in all to about (3 000) *three thousand.*

As to the denial made by the principal English archæologists regarding the genuineness of the jaw described by M. de Perthes, I refer to the report of Milne-Edwards and others in the "Compte Rendus," from May, 1863, to July, 1864.

If we cannot rely upon statements supported by such an accumulation of proof as is there produced, we must refuse credence to most of the reported facts in relation to all other fossil remains.

AGE OF THE ABBEVILLE GRAVEL BEDS.

It may not be true that the superficial materials in which these remains are found, are of the same age as the North American drift. They are, however, the only stratified beds between the soil and the cretaceous strata in that region, and they occupy a very extensive region in France and England. These beds contain the fossil horse, ox, Elephas primigenius, and rhinoceros, and constitute what is known in Western Europe, by the name of diluvium. It is quite probable that those wide spread diluvial layers belong to a modified condition of the drift materials, corresponding to what we observe here in large valleys near the southerly border of the boulder drift. As the glacial period drew towards a close, the intervention of ice as a transporting agent became less, and that of water greater. The European diluvium probably belongs to the later phase of this era.

Some explorers detect the action of large floes of ice, in the movement of blocks belonging to strata foreign to the region; but most of the materials are of local origin, belonging to the valleys of the present streams. In general, the mammalian remains are the same as in the drift of the United States. Along the great valleys of Western Europe, such as the Seine, the Rhone, and the Rhine, the loess is a conspicuous member, generally the upper one. It is the same in the valley of the Missouri, the Mississippi and the Ohio. The European diluvium is a fresh-water deposit, like that of the western and northwestern drift deposits, on the waters of the lakes and the Mississippi. It embraces, like our drift beds, many varieties of timber and vegetables which are more northern than the present vegetation. The fresh-water and land shells which are embraced in the superficial materials here and in Western Europe, are almost identical. So many resemblances go far to establish a geological parallelism.

Mr. Lyell inclines to the opinion that the flint bearing beds of Amiens and Abbeville are more ancient than the bone layers at Natchez, Mississippi, which are at the bottom of the loess.

Whatever may be the exact order of arrangement, there is a general synchronism between the post-tertiary or quaternary

system of Europe and the United States, and since human relics have certainly been found in this formation in the old world, it is reasonable to expect them here. If human bones exist in our northern drift or in the modified river valley deposits of the terrace period, generations may pass before they are discovered. Flint and stone implements are imperishable, but the osseous parts of animals and men are dissolved by chemical action. In beds of such great antiquity, historically considered, as the lower quaternary, very little is preserved except the teeth. Such discoveries must, therefore, be due in this country, principally to accident, and not to research.

We cannot expect to have the concurrent aid of an enthusiast like M. Boucher de Perthes, a government ready to assist, and thousands of workmen turning over the earth, in which such revelations may be expected.

ANTIQUITY OF THE NORTH AMERICAN RED MAN.

It is a little more than three hundred years since the Spaniards landed on the shores of the Gulf of Mexico, where they found the red man, in every respect the same as he is now, among the southern tribes of the United States. If we can credit the historical literature of Iceland, the Northmen were on the coast of New England about the year 1000, where they met a savage people, apparently the same as our forefathers encountered there six hundred years later. The same tribes of Indians, remnants of whom have survived to our day, were seen during the previous one hundred years on the St. Lawrence and the Atlantic coasts, by Jacques Cartier, Americus Vespucius, Verrezani and the Cabots.

By historical proofs that are now regarded as worthy of credit, we can thus trace the North American Indian, as a resident in the territory of the United States, backward more than eight centuries and a half. In this as in all countries there are found the remains of a people more ancient than any written record.

The North American Indian has always occupied a position so low, in comparison with other races, that permanent monuments of his presence are very rare. If he had disappeared before the advent of an historical race, no certain traces of his

existence would have been found. His tenements were temporary shelters, which he carried from place to place. The mounds he raised over the dead were low, and contained relics of which the most that can be said is they belonged to a people in the rudest state of barbarism. Where he cultivated the soil, it was in a manner so slight that a few centuries of time obliterated the evidences of it. The scattered inscriptions and effigies which he cut upon trees and stones had no meaning to any one but himself. His tools and implements had no special characteristics to distinguish them from those of any other savage people in the lowest scale of development. Since the historical period in North America he has remained precisely the same, without progress towards civilization, and therefore it may be inferred that his condition in previous centuries while an inhabitant of this country was a fixed one. He could not have descended much lower, and he has left no evidences of an advance. The effect of this fixedness of condition is to render the deductions of craniologists of more value than they would be in the case of a people whose mode of living, whose thoughts, occupations, dress, religion, intelligence, food, and general surroundings were subject to change, especially in the direction of progress.

How long have they been in the occupation of North America, and how can we determine the period?

The antiquarians of Europe regard those ancient people who used *flint implements*, as having been prior to those who had implements of *stone*, and the latter as being older than the races who had the use of bronze, or other metals, especially iron. In the United States the race next prior to the white one, had very few implements of stone. Their knives and arrowheads, their war implements, and their agricultural tools, were almost entirely of flint.

They had a very few and very rude cutting instruments of native copper; possessing at the same time in a certain degree, implements of the flint, the stone and the metal era of Europe, the flint greatly predominating. The mound builders, who preceded the race of red men, produced and used tools in the reverse order. Their *stone* axes, adzes and mauls, were very numerous, *copper* tools plenty, and those of *flint* very rare.

In their case the most ancient people were the most industrious, and cultivated the soil, possessed more mechanical ingenuity, and left more prominent monuments, which in the course of nature will never perish.

The difference between the relics of the mound builders and the Indians is so great that there is little difficulty in deciding between them.

On the Atlantic coast, from Nova Scotia to Florida, there are numerous refuse heaps of sea-shells, which are almost identical with the ancient shellheaps of Sweden, Norway and Denmark, known in those countries as "Kjœkkenmœddings."

Those of the Eastern and Southern States, have been examined by Mr. Jones, of Halifax, Professor Wyman and others, who regard them as the work of the North American Indians. On one of the shell mounds of Florida, situated on the river St. Johns, there grew an oak, which was five hundred and fifty years old. There are very ancient shell mounds on the Tennessee River, between Chattenooga and the "Muscle Shoals," but they have not been thoroughly explored. I have also seen them on the Wabash in Indiana, near New Harmony. The American shellheaps, like those of Scandinavia, contain stone and flint implements mingled with charcoal, and the bones of animals, fishes, birds and reptiles, now or recently living in the vicinity. Only one small bone of man has been found in the American kitchen-muddins.* They have neither the size or the marks of antiquity, which the Danish shellheaps exhibit but are some of them so ancient that changes of climate and of vegetation have occurred while they were being formed. The shellheaps, therefore, as at present known, give us very little information in regard to the antiquity of the red man.

But there are remains of the same race in bone caves, and rock shelters, which bear directly upon this question, to which I will call your attention.

*Since this paper was read, a human *underjaw*, with teeth, has been found by Prof. S. F. Baird, at the Shellheap on Eagle Hill, Ipswich, Mass. For an account of this and other Shellheaps in New England, see Prof. Wyman's articles in the "American Naturalist," Vol. I. Later investigations than those reported by Prof. Wyman have shown the deposit at Eagle Hill to be much larger and deeper than was at first supposed.—EDITOR.

ELYRIA SHELTER CAVE.

Examined in April, 1851, by Dr. E. W. Hubbard, Professor J. Brainerd, and Charles Whittlesey.

This is one of numerous instances where the "grindstone grit" of northern Ohio, resting upon soft shale, presents a projecting edge, forming a grotto capable of sheltering a large number of persons, being about fifty feet in length, by fifteen feet broad. This and others in the vicinity which have not been explored, correspond to the European "shelter cavern," where human remains are always found. These retreats constituted the domicils of our race, while in their rudest condition. We dug to a depth of *four feet* on the floor of this cave, composed of charcoal, ashes, and bones of the wolf, bear, deer, rabbit, squirrels, fishes, snakes and birds; all of which existed in this region when it became known to the whites. The place was thoroughly protected against rains. At the bottom, lying extended upon clean yellow sand, their heads to the rear and feet outwards, were parts of *three* human skeletons; two of them nearly entire. Two of the skulls were preserved by Prof. Brainerd. They were decided to belong to the North American race of red men, by those who had an opportunity to examine them. These skulls were exhibited at the Cincinnati Meeting of the American Association in 1851, but were afterwards destroyed by a mob, together with the entire Museum of the Homœopathic College, at Cleveland. The position of the skeletons indicated that they were crushed by a large slab of the overhanging sandstone falling upon the party, while they were asleep at the back part of the grotto. One of the skulls was that of an old woman, the other of a young man. Flint arrowheads such as the Indians once used, were scattered throughout this mass of animal remains.

This cave may be found on the west bank of Black River, a short distance below the forks, at the town of Elyria, Ohio, in a romantic gorge, through which the river flows. Judging from the appearance of the bones, and the depth of the accumulations over them, two thousand years may have elapsed since the human skeletons were laid on the floor of this cave.

HUMAN REMAINS IN A CAVE NEAR LOUISVILLE, KENTUCKY.

T. R. Scowden, C. E., 1853.

In constructing the reservoir for the Louisville water-works, on the bluffs of the Ohio, two miles above the city, the Engineer, T. R. Scowden, Esq., discovered a cave in which were a large number of human bones. It is forty feet from a mural face of lime-rock of the Upper Silurian epoch, which is known in Kentucky as the "cavernous limestone." The elevation of the bluff is about one hundred and twenty-six feet above low water in the river, and ninety feet above the bottom lands, which are half a mile wide in front of the water-works. It is probable the cave is an extensive one. No outlet is known, and when water was directed into it, no place of discharge was discovered. As far as it was explored the opening was not large. It had a direction downward and to the rear, but was so much infested with rattlesnakes that no one could be induced to examine it. On the rock there was ten feet of the loess like loam of the country, in which was a depression, into which the surface water settled, such as in that region are called sinks. The bones, a box of which were preserved by Mr. Scowden, were cemented into a breccia, by calcareous drippings from above. In one mass there are portions of six human crania, but none of them large enough to be of value in the comparison of races. There are other bones and teeth, representing more than that number of persons, which are in a good state of preservation. The opening in the rocks at the top of the cave, which was closed by a loamy clay, was not as large as the cavern, the roof of which was twelve feet below the surface of the lime-rock. From the roof there were the usual pendant concretions, known as stalactites. In shape this part of the cave was a dome, six feet across at the base, and about five feet high, the bones lying in a confused heap on the floor. The downward passage into which the water flowed was situated at the rear, and its direction was away from the bluff. A stone axe and a pestle were found with the bones; also a flint arrowhead. Below the cliff there was an ancient Indian burying ground, in which many graves and human bones were exposed while digging the trench for the main inlet pipe of the water-works.

The bodies may have been introduced for burial, through a distant entrance not yet discovered, or there may have been a time when the cave was open above. They were evidently of the Indian race, and the place was a sepulchre. Among the Hurons who lived between Lake Ontario and Lake Huron, when the French missionaries were there, two centuries since, there was a practice of collecting, from time to time, the bones of their dead from all the graves of the tribe. They were then placed in a pit, without order, and covered in the presence of all the people, consecrated with funeral ceremonies and lamentations. The cavity or sink in the earth, at Louisville, would constitute a burial pit already made, or partially made; and after the bones were deposited, they could have been easily covered. From the quantity of tufa formed on the roof and over the bones on the floor, it is evident that a long period has elapsed since they were deposited—full as long as in the case of the Elyria grotto, or say two thousand years.

HIGH ROCK SPRING, SARATOGA, NEW YORK.

Profile by Dr. Henry McGuire, 1866.

1.	Muck and tufa on which the cone rested . .	7 feet.
2.	Tufa	2 "
3.	Vegetable muck, on the surface of which lay a pine tree, with one hundred and thirty annual layers of growth, worn by the feet of persons, probably Indians, ...	not given.
4.	Calcareous tufa, same as No. 2 . . .	3 feet.

Chancellor Walworth states, that in 1825 he saw at St. Regis, New York, an old Mohawk, by the name of Loren Tarbel, who said the water did not flow over the cone when he was a boy.

The estimates made by Dr. McGuire, of the time occupied in the formation of the tufa, which showed twenty-five layers to the inch, is, for five feet, fifteen hundred years.

Rate of formation of the rock cone, eighty years to the inch, is, for four feet, equal three thousand eight hundred and forty years (probably too large). For the accumulation of muck, five hundred and ten years (which is probably too small). Total, five thousand eight hundred and forty years. The pine tree

worn smooth by the feet of men, beneath the upper bed of tufa, must have been there at least one thousand years before the formation of the rock cone.*

Mr. Koch, who furnished a skeleton of the *Mastodon Ohioensis*, from the recent alluvium of the Pomme de terre River, Mo., to the British Museum at London, convinced the English geologists, that he found a flint arrowhead at the depth of fifteen feet beneath the skeleton, which arrowhead was of the pattern used by the North American Indians. He also stated that near the skeleton, and full as deep, were three other flint arrowheads. If these statements are reliable, they tend to extend the antiquity of the occupation of red men much beyond that of the American shellheaps, in which are no remains of extinct amimals. This statement of Mr. Koch, is, however, contradicted by one of the men who assisted him in exhuming the skeleton. A similar case is presented by Dr. Holmes of Charleston, S. C., in the "Proceedings of the Philadelphia Academy of Natural Sciences," for July, 1859. He found pottery at the base of a peat bog, on the banks of the Ashley River, in close connection with the grinder of a mastodon.

This pottery probably belonged to the red man, and if so, strengthens the proof of his presence here before the horse, mammoth and mastodon were extinguished.

A PEOPLE BETWEEN THE INDIANS AND THE MOUND BUILDERS.

The ancient earthworks which abound on the waters of the Ohio do not extend northward to Lake Erie. There is a belt of country north of Central Ohio, near the water shed of the streams that empty into the Lake, which is without ancient works, so far as at present known. On all the rivers discharging into Lakes Erie and Ontario from the South, there are ancient forts in profusion, but they are of a type entirely different from those in the valley of the Ohio, which extend southward through Kentucky, Tennessee and Texas, into Mexico.

*Dr. McGuire has republished an account of this excavation at High Rock Spring, in the "Proceedings of the Boston Society of Natural History," Vol. xii, p. 398 (May, 1869), more in detail than the above, but giving nearly the same total result (5470 years, within all bounds). He also mentions the finding of a fire-place and charcoal on the clay bed, under a stratum of muck 2 feet thick, underlying the last 3 feet of tufa mentioned above.—EDITOR.

There are few mounds and no pyramids or rectangles on the shores of these lakes, but hundreds of small irregular fortifications of earth. They are always located in places which have natural strength, such as bluffs and points of land with water near by, and steep ravines on two or more sides. Generally the fort consists of a ditch or ditches, and embankments across the narrow part of a peninsula. They are almost without exception well selected strongholds, judiciously fortified, and have the appearance of long occupation. The people who occupied these forts doubtless comprised not merely soldiers, but the entire population of the country. They must have been cultivators of the soil, but divided into hostile clans, like the old Scots and Germans, who required castles as a defence against each other. This race of fort builders on the lakes may have been contemporaries of the mound builders, and of the effigy builders of Wisconsin; but they were of prior date to the Indian, who has no more knowledge of the origin of these forts than of the mounds, and who had no earthworks when the whites first encountered them. Mr. Squier in his "Antiquities of Western New York," attributes them to the Indians, but upon grounds that do not seem to me sufficient.

I introduce this intermediate, or if not intermediate, distinct race, as the basis of future investigation; but do not feel warranted at present, in using it to increase the antiquity of the American man. I confine myself to the red men, the mound builders, and to the evidence of fossil men, contemporary with the elephant, mastodon, horse megalonyx, and other mammalia of the quaternary.

THE RACE OF THE MOUNDS.

The difference in their modes of burial, indicates clearly that the mound builders were a people distinct from the North American Indians. In almost every ancient burial mound, the remains of both races have been interred; the bodies of the most ancient at the bottom, or at great depths, on charcoal hearths, in rude enclosures of wood or stone, with copper ornaments, implements, wrought shells, coarse cloths, and other peculiar marks. On the sides of the same mounds, some of which are seventy feet in height, are the skeletons of the

red race, at shallow depths, with no attempt at stone or wooden coffins, and in the early graves no metal ornaments. In the sepulchres of the mound builders, flint knives are rare, and flint arrowheads still more so; but stone axes are not uncommon. In the Indian graves are numerous flint arrowheads and knives, but stone implements, except pipes, are very rare. The Indian has nowhere raised conspicuous earth mounds over his dead. Their most notable monuments, are low stone-heaps, like the Irish cairns, formed gradually by the friends and admirers of the deceased, as they pass the spot, throwing a small boulder upon it. In the Indian graves are no evidences that the body was burned, as there are in large numbers of the old earthen *tumuli*. No instance is reported where an earth mound has beneath it the remains of a race which might be more ancient, but the instances are numerous where there are bones of the mound builders beneath those of the red man.

The mound builders show in all their relics and their work, a greater mechanical skill, more intelligence, industry, and perseverance, than the Indian tribes ever possessed. They wrought the copper mines of Lake Superior very extensively for the purpose of fabricating tools of this metal, which are found in their mounds, in contact with their skeletons. The Indians had only the rudest copper implements, and these were of a pattern quite different from those discovered in the mounds. It was a very rare circumstance that the French missionaries found among them copper that had been wrought. It was generally treasured up as a manitou, in the original form of a nugget, and transmitted as an heirloom from generation to generation. The copper adzes, axes, chisels, spades and spearheads, of the mound builders, were nowhere found in use among the living tribes, nor any tools having the same degree of finish, or intended for purposes of so high civilization. They had only the rudest knives, arrowpoints, and implements for curing skins.

These facts sufficiently attest the presence of different races, and at a different period; the builders of temples and burial mounds being the predecessors of those who constructed no monuments or fortifications of earth. We must also infer that

a people sufficiently numerous to work the mines of Lake Superior throughout the copper region, and to construct works such as those at Newark, Marietta, Circleville, Portsmouth and Cincinnati, must have been permanent occupants; but how shall we determine the length of this period of occupation.

After examining the principal works of the mound period, in Ohio, and their mine works on Lake Superior, I could not estimate the period of their occupation at less than one thousand years, with a strong probability in favor of two thousand. If we drop for the present the supposition of an intermediate race, and take the highest estimates for the occupation of the red man and the mound builders, we have only four thousand years, which does not carry us back to the beginning of the historical period, in Asia and Africa.

In Switzerland, relics of man in the recent alluvium attest his presence there, according to M. Morlot, nine thousand to eleven thousand years, or beyond the historical records of the old world several thousand years. It is highly probable, and is in accordance with the analogies of Europe, that since the glacial period, there was a people here, more ancient than the mound builders, but I know of no remains of such a people, except the charcoal beds at Portsmouth, Ohio, in the ancient valley alluvium. The whelk, found by Mr. Cleveland, is a shell which was in common use by the mound builders. The alluvium in which it was embedded, is of the most recent kind, which at the mouths of the tributaries of the Ohio, forms very rapidly. At the mouth of the Great Miami, the mud deposited from back water in a single flood of the Ohio River is sometimes several inches in thickness. Since the settlement there in 1789, the stumps of trees and large logs have been covered up in this way, until the plough now passes over them. This modern alluvium is easily distinguishable from the ancient river alluvium, which is derived from the drift materials, and which must also be distinguished from deposits of the terrace period, known in Ohio as the valley or modified drift. The terrace period represents the closing phase of the glacial era, when the valleys were full of water and floating ice, sorting the materials on a broad scale, in lines parallel with the streams. The ancient river alluvium is due wholly to the currents of

streams acting upon the valley drift of the terrace period, at levels higher than the present channels.

In both these deposits the elephant and mastodon are common, as well as in the recent alluvium. In the ancient river alluvium has been found the gigantic beaver, or *Castoroides Ohioensis* of Nashport, Ohio, the taperoid jaw of Yellow Creek, Columbiana County, Ohio, and probably the *Bos bombifrons* of Trumbull County, Ohio.

There is, therefore, a long period of time to be accounted for between the earliest mound builders in this country, and the earliest inhabitants of the Swiss Lakes and of the Nile.

Since the close of the terrace period there has been no material change in the surface of Ohio, and perhaps no general change in the climate. Man could have resided here then as well as now. In Belgium and France relics of man extend much farther back, into and even beyond the terrace period, when the surface of the country was somewhat different from what it is now. He lived, and may have perished, with the elephant, mastodon, rhinoceros, and the extinct elk, during the closing portions of the ice period. It is therefore reasonable to suspect that man existed in North America with the extinct elephant, mastodon, megalonyx, horse, beaver, and the peccary of the United States, which lived towards the close of the ice era, though it does not follow that they, and he, were exactly cotemporary here with the European species.

With the exercise of the same never tiring research displayed by M. de Perthes, and the same facilities, it is highly probable that the drift clays of Lake Erie, and the valley of the St. Lawrence, would furnish us with equally palpable specimens of the ancient man.

EVIDENCES OF MEN MORE ANCIENT THAN THE MOUND BUILDERS.

In 1838 while examining the structure of the fluviatile deposits on the Ohio River, at Portsmouth, Ohio, I saw in two places in the east part of that town, the remains of very ancient fires.

At low water, and thence up to a height of twelve and fifteen feet, is a bed of sand and transported gravel, containing pebbles of quartz, granite, sandstone and limestone, derived partly from

the adjacent carboniferous and Devonian rocks, and partly from the northern drift, the upper part much the coarsest.

On this is a layer of blue quicksand, from one to five feet thick, in which is a timber bed, including large numbers of the trunks, branches, stumps and leaves of trees, such as are now growing on the Ohio, principally birch, black-ash, oak and hickory.

Over the dirt bed is the usual loamy yellow brick-clay of the valley, fifteen to thirty feet in thickness, on which are very extensive works of the mound builders. In and near the bottom of this undisturbed homogeneous river loam, I saw two places where fires had been built on a circular collection of small stones, a part of which were then embedded in the bank.

The stones were colored red by heat, and among them was charcoal, covered by the clay, of which I have specimens. Around and near to the fire beds, were what appeared to be the exterior membrane-like covering of river shells (unios), but no shells. It was several rods from one of the charcoal beds to the other, and they were not precisely on the same level. They were from eighteen to twenty feet above low water, and about fifteen feet beneath the surface. There are no trunks of trees in the loamy brick clay which is not laminated. It was reported that some of the trees in the blue stratum below had been charred. On the surface of the clay deposits near the fire beds were two parallels, portions of ancient earthworks extending to the river. At the west end of the town—where an artificial mouth of the Scioto was formed, about thirty-five feet deep, in order to allow the Ohio canal to enter the river—the blue quicksand bed and the loam above it dips westerly down to the level of low water. To the westward of this artificial mouth the recent alluvium of the Scioto overlies the yellow loamy clay, cutting off both the quicksand and the loam. The Scioto alluvium has a darker color, is frequently contorted and laminated, and has embedded leaves and branches of trees now growing on its banks.

Francis Cleveland, Esq., an engineer upon the State works, who, in 1828, had charge of the deep cut, informed me that at a depth of twenty-five feet in this alluvium several conch shells were taken out, which were five to eight inches in length. He

said they were the same as one I then exhibited to him, procured from an ancient mound on the Scioto River, and which Professor Kirtland determined to be a *Pyrula perversa* from the Gulf of Mexico and Chesapeake Bay, and called whelks. These shells were in common use by the mound builders, probably in their public ceremonies.

Here we have within the limits of the city of Portsmouth, memorials of the mound period, and, as I conceive, of the rude fires of men of still higher antiquity.

REPUTED EVIDENCES OF MAN IN THE QUATERNARY.

Profile of the Mississippi River Bluffs, at Natchez, Mississippi, by Professor C. S. Forshey, 1842.

1. At low water mark, slate, thickness unknown, probably cretaceous, . . 5 feet.
2. Recent conglomerate, probably quaternary, 8 " = 13 feet.
3. Coarse gray water-washed sand, quaternary, (high water mark at 40 feet), . . 115 " =128 "
4. Yellowish brown homogeneous calcareous loam, with fresh-water and land shells, the equivalent of the loess of the Rhine, 50 " =178 "

On the surface of the sand, the skeleton of a mastodon was found, which appeared to have been buried erect.

Sir Charles Lyell in 1847 (Antiquity of Man, p. 200), collected at this place twenty species of *Helix*, *Heliciana*, *Pupa Cyclostoma*, *Achatina* and *Succinea*. And in the marly layers *Lymnea*, *Planorbis*, *Paludina*, *Physa* and *Cyclas*.

A narrow gully called the "Mammoth Ravine," near Natchez, originated in the convulsions of the earthquake of 1811, is seven miles long and sixty feet deep.

The *Mastodon Ohioensis* in the bed next below the loess was found in connection with the horse, Bos, Megalonyx, and other mammals.

Among these bones Dr. Dickson found the pelvic bone of a man. It was colored black, like the other bones of this bed, but so are the recent bones of Indian graves in the neighborhood. The superficial materials of the Somme in Picardy may be older than the Natchez bone bed, but there is not reliable evidence that Dr. Dickson's *os innomanatum* belongs to this

bed. It is a case open to investigation without prejudice, like the early discoveries of Boucher de Perthes.

Sir Charles Lyell's profile at Vicksburg, Mississippi, eighty miles north of Natchez, 1846.

1. Below low water, cretaceous.
2. Sand-bed with rolled pebbles, same as at Natchez. [This he regards as Eocene tertiary, but possibly drift. No tertiary has been reported by American geologists along the river above Vicksburg.]
3. Calcareous loam or loess, same as at Natchez.
4. Soil.

The loam, or loess of the Mississippi, has since 1846 been traced by Professors Safford, Owen, Swallow and Worthen, from Vicksburg to the mouth of the Missouri, and up the Ohio beyond the Wabash, where it contains the Megalonyx.

WORKS OF ART, GRINNELL LEADS, KANSAS.

An instance is given by Professor Daniel Wilson (Prehistoric Man, p. 46) of a flint knife, found at the Grinnell leads, Kansas, by Mr. P. A. Scott, at the depth of fourteen feet. It is probable that this knife belonged to some of our Indian races, with whom flint implements predominated, but how ancient the overlying deposits are, cannot be considered as well settled.

There is, however, a case given, upon the authority of Professor Agassiz, where the jaws, teeth and bones of the human frame were found by Count Pourtales in a calcareous conglomerate in Florida, which is geologically recent, but which they consider required ten thousand years to accumulate over the bones.* As no opinion is given as to the race to which these remains belong they cannot be connected with either the red man, or the mound builder. They may be parts of a skeleton, drifted from far distant lands, especially from the islands which constitute the West Indies. Unfortunately there is not only

*Count Pourtales, in reference to several erroneous accounts that have been given regarding the Florida bones, makes the following statement in the "American Naturalist," Vol. ii, p. 443 (Oct., 1868). "The human jaw and other bones, found in Florida by myself in 1848, were not in a coral formation, but in a fresh-water sandstone on the shore of Lake Monroe, associated with fresh-water shells of species still living in the lake (*Paludina, Ampularia, etc.*). No date can be assigned to the formation of that deposit, at least from present observation." — EDITOR.

in this but in all the cases quoted in the United States, pertaining to the quaternary period, a degree of uncertainty in the evidence, which is fatal to a scientific result.

The human skeleton described by Dr. Dowlais, found at the depth of (16) sixteen feet in the city of New Orleans, to which a high antiquity has been ascribed, belongs, according to the later investigations of Professor Hilgard, of the survey of Louisiana, to the recent alluvium.

While the canal around a rapid of the St. Lawrence was being excavated near Brockville in Canada, Dr. J. Raynolds of that place procured several copper tools, which are reputed to have been found (14) fourteen feet below the surface (Smithsonian Contributions, vol. x, p. 208). There were at the same depth a number of human skeletons, placed there evidently, according to some form of sepulture. The copper spearheads found among these skeletons correspond in their general characters to those of the mound builders, except in the mode of attachment to the shaft. In the Brockville spears there is a pointed spike or shank, instead of the usual hollow socket. There was also a spade-like tool of copper, of which none have been found in the mounds, but I have seen one from the drift gravel of Lake Superior closely resembling it. Neither have the copper spearheads, attributed to the mound builders, been found at such depths in their works as to render it certain they were made and used by them. The age of the Canada formation is not determined, but appears to be of the later drift, or terrace period, and the remains may have received a part of their covering from the alluvial wash of higher lands, or from slides. This locality requires farther examination.

On the Ashley river, near Charleston, South Carolina, Dr. Holmes reports an instance where he discovered fragments of pottery at the bottom of a peat-bog, in close connection with remains of the Mastodon and Megatherium (Proceedings of the Philadelphia Academy of Natural Sciences, July, 1859). There is here a close resemblance to the pottery finds of the deposits of the Nile, and the relics of the peat bogs at Abbeville in France. There is in them evidence of high antiquity, not, however, carrying us beyond the alluvium.

As the pendant or "plumb bob" of Sienite procured by Pro-

fessor Grimes at the depth of (30) thirty feet below the present surface, in the bed of an ancient lake in California, will doubtless receive a notice in these "Proceedings," from Colonel Foster, I make no farther reference to it than to call attention to the fact that the deposit must be regarded as more recent than the drift. Mr. John Collot, of Vermilion County, Illinois, has in his possession a similar ornament or implement three inches long, made with great care and symmetry from a piece of pure crystalline specular iron ore. He has also a fragment of another precisely like the above, and a beautifully formed hemisphere, like a paper weight, fabricated of the same material. They were found on the surface about fifteen miles south of Covington, Indiana.

Near Perryville, in Knox County, Ohio, about twenty years since, I. N. Pillsbury, Esq., Civil Engineer, of Cleveland, Ohio, discovered a "plumb bob," very like the Sienitic one of California. It was taken by him about one foot beneath the surface, within one of the ancient forts common in Ohio. The material is a whitish gray crystalline limestone, not as elegant in form as those of Mr. Collot and Professor Grimes, but about the same length.

In all of them, the hole at the upper end through which they were suspended, tapers towards the centre. It is more likely they were amulets or ornaments worn about the person, and are of an era subsequent to the mound builders.

In this epitome of the evidences of man in the United States prior to the historical period, I have not alluded to the reputed skulls of Calaveras County, California, produced respectively by Professors Whitney and Blake. In regard to both of them there is a direct contradiction in reference to the authenticity of the relics, and the age of the deposits in which they are supposed to have been found. These "Proceedings" will no doubt contain their views and their proofs at full length. In every instance where we descend below the alluvium in search of human remains and relics we are thus far met by conflicting testimony as to the facts.

The later clay, sand and gravel beds of the drift era on the lakes, and the newer drift and loess beds of the Mississippi, should contain them, if these deposits are, as they appear to be,

coeval with those of the Somme. In the Belgian caves we are also furnished with works of man that seem to be as ancient as the closing out of the glacial period.

---

2. ARCHÆOLOGY OF THE MISSISSIPPI VALLEY. By W. De Hass, of St. Louis.

THE progress of Archæology has been rapid and successful abroad, but at home little comparatively has been accomplished. In the great valley of the Mississippi *nothing* had actually been done previous to the present season.

Anxious to compare the ancient earthworks of the Mississippi, Ohio, and subordinate valleys, I came West in March, 1868, and as early as practicable commenced operations.

The extensive system of mounds on the American Bottom having long attracted attention, and there being some diversity of opinion as to the character of these interesting monumental remains, I have examined with care these really fine and remarkable works, and respectfully submit, for the consideration of members of the Association, the result of observations and explorations thus far prosecuted.

These researches have been prosecuted in that spirit of scientific inquiry which is pressing forward exploration in other lands; which is not content to rest with mere affirmation or specious speculation, but demands careful, thorough, and laborious efforts to develop the primordial life of man, trace out his remains of art and industry, collect the fast perishing memorials, and otherwise preserve these early monuments and vestiges of art.

The American Bottom, Illinois, presents the most extensive and remarkable system of ancient *tumuli* to be found in North America. Other parts of the country present more varied works, but nowhere are the mounds so large and numerous, and arranged with so much system, as the locality referred to. They exhibit, indeed, a city of mounds, a vast and mysterious collection of monumental remains.

Familiar with the earthworks of the United States, I have been not a little surprised at finding those on the opposite side

of the Mississippi, entirely tumular. Elsewhere they generally occur in connected groups, associated with circumvallations, ramparts and mural works.

Before entering on a description of these interesting monumental remains, with vestiges of art found in and adjacent to them, I will give a brief historical retrospect of the great alluvial known as the "American Bottom." This comprises the most extensive and valuable alluvium in the United States. It stretches from nearly opposite the mouth of the Missouri to the Kaskaskia, over eighty miles, and has an average breadth of about seven miles. Of inexhaustible fertility; abounding in scenery highly picturesque — lakes, creeks, and running brooks — it was evidently the favorite abode of an active and populous community in prehistoric days. The most desirable portion is that lying north and east of Big Lake, or the Belleville road. Much of this is prairie, and the early occupants of the valley selected this as their home, built their mounds and made secure their foothold on the broad domain. The name by which this fine body of land is now known was derived from a settlement chiefly from Virginia, Kentucky and Tennessee, many of them followers of George Rogers Clark. These were *Americans*, and known as such in contradistinction to those of Gallic descent.

At an early day the indefatigable explorers, missionaries and traders, from *La Belle France*, penetrated the valley of the Mississippi and planted the standard of the cross among the native tribes whom they found occupying the country.

When La Salle descended the Mississippi in 1686, the principal native tribes occupying the region embraced within these observations were the Cahokias, Tamaroas and Kaskaskias, belonging to the Illinois confederacy. Allouez soon after founded his mission among the Kaskaskias, and about the same time Gravier and Pinet claim the credit of founding missionary posts with the Caoquias.

Sebastian Rale (Biography in Sparks) visited the Missions in 1692, spending two years among the Illinois Indians. Charlevoix visited the region in 1721, and speaks of the Caoquias and Tamaroas.

These early writers describe the manners, habits and customs

of the Indians, *but are silent about the mounds.* The natives were indolent and dirty, devoted to hunting and fishing. They were acquainted with some of the simpler arts. Charlevoix describes the mode of making thread for sewing their robes, moccasons, etc. "After stripping the flesh from the sinews of the roebuck they expose them to the sun for the space of two days; after they are dry they beat them, and then, without difficulty, draw out a thread as white and as fine as that of the Mechlin, but much stronger."

One of the earliest reliable writers on the antiquities of the American Bottom was Breckenridge. In his "Views of Louisiana," published in 1817, interesting sketches are given of his visit to the Indian and French villages at Kaskaskia and Cahokia. He speaks of a "number of large villages; that a lucrative fur trade was carried on, and their agriculture extensive." "These settlements sent to New Orleans in one year (1746) 800000 pounds of flour, while at the time there was not a single settlement on the western side of the river." The principal villages were Kaskaskia, Prairie du Rocher, Cahokia, De Charte, and St. Philip. The two last have entirely disappeared." (1812–13.)

Breckenridge visited the mounds. He says: "As I approached the foot of the largest mound I was struck with a degree of astonishment, not unlike that which is experienced in contemplating the pyramids."

The Indians disavowed all knowledge of the monuments. They had no tradition concerning their origin or purposes. The old chief of the Kaskaskias told Mr. Rice Jones that in the war of his nation with the Iroquois, the mounds on the American Bottom were used as forts.

From the days of Breckenridge's observations to the present, these interesting antiquities have attracted attention but not investigation. Most books of travel were incomplete without some fanciful notice. The periodical press have repeatedly noticed them. Speculation has been busy as to the origin, character and purposes of these remarkable works, and, worst of all, ignorance, with brazen assurance, has claimed to be heard in expounding the mystery of the mounds. So remarkable, varied and extensive are these monumental remains that

many persons could not comprehend how human labor could have erected them. The Pyramids would doubtless still more baffle and confound these sagacious observers.

A fluviatile origin has been a ready and plausible theory with many persons. Another respectable authority regards them as outliers of loess brought to their present form by denudation.

The writer of an elaborate paper in the "Edinburgh Review," speaking of the great mound says: "This mound is constructed with as much regularity as any of the *teocallis* in the South, and was originally *cased with stone* (some American archæologists maintain *with brick*) and surmounted with one or more buildings," etc. The writer does not inform us who these "American archæologists" are, but possibly they are allied to that class who "maintain" the "geology of the *tumuli*," prate about "stratified mounds," and delight to use scientific terms, the very meaning of which they are utterly ignorant. These are third-rate country doctors, rural district teachers, and other tyros who imagine they have discovered in graphic mica, or encrinital limestone, genuine Hebraic inscriptions.

It is not surprising that novices should commit egregious blunders in attempting to discuss subjects they do not understand; but it is surprising that those whose position and investigations should have induced them to examine carefully the character of these works before expressing positive opinions, have failed to do so. The only charitable conclusion is they never examined the mounds. No man whose opinions are worth quoting could have examined even one of these interesting monuments, and not declared, unequivocally, in favor of artificial origin. The proofs are clear, abundant and conclusive. Externally and internally, character, structure, position and contents, all incontestably prove them the work of man's labor, industry and spirit of combined action. All, from the largest to the smallest, are the result of human agency. On this point there need be no farther cavil or doubt.

With all due deference to very respectable authority in the West I must dissent from his expressed views that these mounds are *loess*. They are totally different in character and composition. The *loess* is uniform buff colored; all mounds alluded to *are a heterogeneous mottled composition* of vegetable

mould, sand and clay. This is the unvarying character of every mound on the American Bottom that I have examined. The great tumulus presents this marked feature, as do the smallest composing this extensive and remarkable system.

But of the mounds—who erected them? How were they constructed? When built? For what purpose designed? Whence came and whither went their builders? These are questions which have baffled inquiry in other days, and still defy our keenest ken. Were their builders Autochthones? or were they immigrants from other lands? How derived their knowledge of agriculture, of working stone, of pottery and other useful arts? How lose all these?

Impressed with the importance of giving these investigations all the attention possible I have labored assiduously to this end. I have traversed the field hundreds of miles, over lakes, across bogs, up creeks, down streams, penetrated its geological strata and climbed long miles of tortuous bluff; have examined, located, measured and mapped over one hundred and fifty mounds, excavating many, and collecting several hundred specimens of ancient art, representing the stone age, the fictilia, the art and skill of the mound builders. This has not been unattended with labor, exposure and expense. But I have the gratification to know that the question of the mounds —whether natural or artificial—has been forever settled. These explorations also afford another pleasure: they enable the archæologist to read from the great book spread out in the mounds and their contents much of the history, character, habits and customs of the people who occupied these broad valleys in long anterior years. And yet the work has scarcely begun. A vast field for scientific exploration opens to the enlightened energies of the Government, of societies and individuals. It is indeed discreditable to our intelligence as a people that these explorations were not made years ago. Many of the finest monuments of antiquity, here and elsewhere, have already disappeared, and the hand of vandalism is being laid violently upon others. The effacing agencies of time and man are rapidly consigning many fine works to irretrievable destruction. Shall this condition of things continue? Better allow the English Societies, as suggested by a writer in the "Edin-

burgh Review," to organize a commission and explore these American monuments; or, as has been suggested in Paris, that the Emperor send a commission to explore the mounds and carry off the implements of industry and of the chase, the weapons of war, etc., which may be discovered. Far better this than that these fine works shall go to destruction and their vestiges of art be scattered and utterly lost.

Two grand groups of ancient *tumuli* loom up on the broad surface of the American Bottom. They are distant from the central figures about six miles, but connected by a series of smaller mounds, forming a continuous chain, and constituting one grand and extensive system of tumular works — unequalled for size, number and interesting feature on either the sub-continents of America.

One of these groups stands within the city limits, and adjacent to East St. Louis; the other six miles to the north-east, lying chiefly north of the Ohio and Mississippi Railway. These are connected by a series of tumuli, stretching along Indian lake and Cahokia creek, the entire system, including those along the bluff numbering over two hundred mounds.

These, collectively, present a vast city of mounds in ruin. They undoubtedly constituted the seat of a great power — a community, little less populous, perhaps, than that now centering within an area of twenty miles of this great modern metropolis of the West. The upper group, containing the most important monuments, was doubtless the citadel of the ancient empire. It comprises over sixty mounds, arranged with great system, and in marked position towards each other. The great mound, constituting the principal feature, is supported by four elevated squares, and numerous large tumuli of manifest importance in the system.

The mounds comprising these respective groups are conical, ellipsoidal, square, parallelogram and paræform. Some are perfect cones, others frustum. They vary in height from five to ninety feet, in some instances presenting an angle of nearly sixty degrees. They are all of earth taken from the surrounding plain or bluff, and constructed with symmetry, neatness and manifest design.

It is claimed as a noticeable fact that corresponding excava-

tions can be observed near most of the mounds. I have noticed this quite marked in some instances, but only in localities where the vegetable mould was found underlaid with a deposit of sand. With their rude implements and facilities for removing soil the mound-builders could not make heavy excavations, but would rather avail themselves of the most readily removed.

I have failed to detect near any of these mounds the fosse so frequently noticed near Ohio valley tumuli. They correspond in general external appearance, internal structure and arrangement to the ancient tumuli of other parts of the country, except those of an elliptical type. This class occurs more frequently here than elsewhere. The square mounds find counterparts in the elevated squares at Marietta, Ohio.

I will not attempt to describe in detail this extensive system, but propose merely a general running sketch, reserving for another occasion a more particular description.

A general design is manifest in all the ancient earthworks of America. In the Ohio valley they are found in connected systems. In the Mississippi valley, or that part lying opposite the city of St. Louis, they occur alone in tumular erections, arranged in groups, with outstanding guards, system and unmistakable design.

It is difficult at the present stage of these investigations to determine the true position in archæology of these Illinois mounds. They may be older than other works in the Ohio and subordinate valleys. They certainly present a distinct type. I have not discovered in this extensive field a single circumvallation, hill or valley fortification, nor the semblance of a mural work.

The remains of art found among these mounds — stone implements, fictilia, etc. — indicate a knowledge, quite equal, if not in advance, of art remains from the mounds of Ohio, West Virginia, Kentucky, Indiana, etc. There is a decided difference between some of their stone implements which will be more particularly noticed hereafter. This fact induces the belief that they belong to different people. As to the object of the mound, without attempting to advance an hypothesis based on incomplete observations, it may be safely assumed

that all mounds wherever, whenever, or by whomever constructed, were *primarily* designed as places of sepulture. This we read alike in the simple and often scarcely distinguishable tumuli in the valley of the Mississippi, or the isles of Britain, as we do in the huge mounds on the Cahokia, or the vast earthen and megalithic monuments of Northern Europe, or the valley of the Nile. They were often devoted to other uses, but the great first purpose was sepulchral. They doubtless often served a triple purpose—tomb, temple, dwelling place. The large square works possibly supported the houses of important personages, or picketed, served as places of defence. The great mound probably supported the principal temple, also the house of their cazique or king. Others served as guard posts, and still others as places of defence.

Many of the larger tumuli are believed to have contained secret entrances which were used after the structure had been completed. This was the case of the great mound at Grave Creek, Virginia, a mound near New Madrid, and it is a noticeable feature in the Pyramids. Mrs. Hill, who resided a quarter of a century on the great mound, informs me that a large entrance was at one time discovered, but filled up to prevent wild animals making therein their dens. Most of the large sepulchral mounds of the West contained originally rude chambers of wood and stone combined. The mound builders did not understand the principle of constructing the arch. In no instance has it been discovered in ancient ruins north of Mexico. The nearest approach was the horizontal or overlapping arch. As the great mound is the largest and most important in the United States, it may be interesting to describe it briefly.

This huge structure—the largest in the United States—stands on the south side of Cahokia creek, two hundred yards from the stream, in the midst of an extensive and most remarkable group of mounds of various size and shape. It was doubtless originally an immense tetragon, supported by a heavy terrace on the south and west, approached by a talus. The destructive agencies of wind and water, uprooting of trees and modern vandalism have much defaced this vast and most interesting work.

Its present dimensions are: north base, five hundred and sixty feet; south base, seven hundred and twenty feet; summit, length, three hundred and ten feet; breadth, one hundred and forty-six feet.

The north side is much the most precipitous. The terrace approaches from the south and west, and is in depth one hundred and twenty feet. The talus approaches from the south, is fifty-five feet broad at top, one hundred and twenty feet in length, and one hundred and twenty-five broad at base. Perpendicular height of mound, as nearly as can be ascertained, ninety-one feet. It was originally, even within the historic period, considerably higher. The base covers nearly six acres. The solid contents have been roughly estimated at twenty-five millions cubic feet. The Pyramid of Cheops covers nearly thirteen acres.

The great mound was originally surmounted by a conical mound ten feet in height. This peculiar feature characterizes one of the square elevations west of the great tumulus. Removal of the small mound revealed numerous interesting relics — human bones, stone implements and weapons, vases of unburnt earthen-ware, etc. These, with a large collection of ancient art were long kept by Mr. Hill, former proprietor of the mound.

The material composing this imposing work, corresponds with that entering into the smaller mounds—vegetable mould, clay and sand. Wherever cut into, and numerous excavations have been made for well, cellar, cistern, ice-house, etc., the same unfailing features are present — a heterogenous mixture of humus, sand and clay, as would occur in removing the surface of lowlands and making slight excavations into the underlying strata. The earthworks of the West have been built in this manner, and when placed on or near alluvium, always show the mottled composition. The great mound at Grave Creek, Va., strikingly illustrates this feature.

The former proprietor of the mound under consideration sunk a well from the steppe, and throughout its entire course threw out broken pottery, fragments of flint, etc. At twenty-five feet, charcoal and partly burnt wood (paw-paw) were discovered. At the horizon of the surrounding plain he passed

through a distinct line of dark carbonacious matter which was clearly the original sward upon which the great tumulus had been erected. Precisely similar indications were noticed by those who excavated the Grave Creek mound and which can be seen at this day.

It may be remarked that water was not struck in this well above the ordinary level, as has been alleged by those who would disprove the artificial character of the mound.

This gigantic tumulus was defended by four elevated squares placed respectively, one on the east, two on the west, and one on the south-west. They vary from twenty to thirty feet in height, and from two hundred and fifty to three hundred feet square. The one on the east is defended by a mound at the south-west corner, and one of those on the west is surmounted by a mound similar to that which crowned the great tumulus.

This imposing monument is popularly known as "Monk Mound;" a decided misnomer, applied because a family of the order of Trappists, located in the vicinity in the early part of the present century. They did not, however, occupy the great mound, but dwelt upon one of the elevated squares immediately west of the larger structure. The author of "Views of Louisiana" visited these rigid religionists about 1813, and thus refers to them: "They at present occupy four or five cabins built upon an Indian mound about fifty yards [feet] high and one hundred and fifty feet *square*." . . . "The other buildings, cribs, stables, etc., ten or fifteen in number, are distributed on the plain below." . . . "They number about eighty persons. It is about a year since they fixed up this place. They had a kitchen garden on the apron of the great mound." The Trappists remained on the American Bottom until 1819, when, after considerable reduction in numbers by death, they quitted the place forever. The mound, with four hundred and twenty acres of adjacent land, was purchased by T. Ames Hill, a native of Massachusetts, but long a resident of Kentucky and St. Louis. He moved on the property in 1831 and erected a cabin upon the very summit. He there lived many years, died and lies buried at the north-west corner of the great tumulus. From his widow, who still survives in

vigorous health at the age of seventy-five, I have derived many interesting facts. She resided twenty-five years on the mound, and now lives near Collinsville.

Of the other mounds comprising this interesting group it will be impracticable, in view of the space already occupied, to more particularly describe them. Some are quite remarkable for size, character and design; several are evidently located as defensive works to the great mound. The whole present an admirable system, and sensibly impress us with their extent, grandeur and the spirit of combined power which influenced their builders. Nothing but a diagram can properly represent their position, connection and arrangement. This, I regret, that pressing personal engagements will prevent my preparing in time for the Association. When brought under the instrument, instead of standing without relation, they are found perfectly in line, or in position to each other. The bluffs are also found lined with tumular erections, and everywhere show they have been used as burial places. The common red man may have availed himself of these commanding points, as he did of the mounds, but still there are evidences of more ancient occupancy. Some of the bluff mounds are quite remarkable. The Sugar Loaf, in Madison county, stands perfectly isolated upon the verge of an elevated plateau, commanding from its lofty summit a view of perhaps forty miles. This structure is a sharp cone, over forty feet in height, at an angle of nearly sixty degrees. A similar work, bearing the same name, looms up upon the bluff in Monroe county. Between these distant points a great number of other mounds occur, some quite high, and generally commanding extensive views. Truly, we are in the midst of a grand system of prehistoric remains!

I have said that the mounds were intended primarily as a place of sepulture; but it must not be understood that all mounds were so used, or that they were places of general burial. They were memorials to important personages, and often, doubtless, family sepulchralia. Western tumuli may be classified as mounds of *sepulture*, mounds of *defence*, and mounds of *observation*, besides those of a miscellaneous character. All were frequently used for burial. Mounds also served a double and triple purpose. The masses were not buried in

mounds. The bluffs and lowlands in the vicinity of the tumuli show extensive cemeteries. In the vicinity of the great mound, human remains, pottery, implements, etc., are dug up almost everywhere, at the depth of two to four feet.

Burial was practiced by the early nations on the Mississippi in at least three different modes; *inhumation*, by placing the body extended at length in a cist, either stone-lined or plain; *inhumation*, by placing the body erect, or in a sitting posture; and *cremation*, or burning the body and placing the ashes and carbonized bones in an urn. Parts of large vessels, which, from their position and other circumstances, were clearly funereal or cinerary urns, have been discovered in the course of these explorations. I hope yet to discover entire urns in the progress of these researches. These modes of burial were observed alike in the mounds and elsewhere.

Minor remains of ancient art constitute an instructive feature in these explorations. I have a large number of specimens, collected from mounds and other ancient depositories, or from the vicinity of them. These represent the stone age, as Scandinavian archæologists divide the progress of human labor. Metallic remains have not as yet been discovered in the mounds of the American Bottom. Copper, in its native state, was the only metal known to the mound era.

The builders of the works under consideration had attained great excellence in working stone. Some of the specimens obtained are of exquisite workmanship and faultless design. It is marvellous how, with their rude tools, they could have fabricated these fine specimens. The chisels and heavier implements, of which I have discovered some quite anomalous, are of granite, hornblende, nephrite, and other varieties of the hardest amphibolic rock. The arrowheads and spearpoints, knives, fleshing instruments, etc., are of quartz of almost every style, from the dull black of the common chert through all the gradations, up to the most beautiful opalescence of the chalcedonic varieties.

One type of these implements is most remarkable. These are *agricultural*, proving beyond doubt that the people who used them tilled the soil. Two distinct styles prevail; one long, like the blade of a spade, and the other identically

our modern hoe, the eye being substituted by a double notch. These vary in size, the longer from six to fifteen inches, and four or five inches broad; the other about the diameter of an ordinary garden hoe. These implements show usage, the parts entering the soil being highly polished, such as nothing would so readily effect as attrition in sand and loam. These implements are quite anomalous; nothing of the kind having been discovered in the Ohio valley, or, so far as I am aware, east of the Mississippi, or in Europe.

A variety of other flint implements have been discovered, unlike any heretofore found. Also, a large granite implement which may have served for dressing hides or crushing corn. I notice, however, a total absence of steatite and serpentine ornaments and amulets, so common in Ohio valley mounds. The implements, ornaments and weapons in stone, indicate two classes — one representing the *palæolithic*, or undressed stone age! the other, the *neolithic* or polished stone age of Sir John Lubbock.

The fictilia from this important antiquarian field is varied and extensive. Vast quantities have been ploughed up and otherwise developed since the first settlement of the valley, but, sharing the common fate of such accidental discoveries, have not been preserved.

The ancient potter in the valley of the Mississippi was but little inferior to his brother of the plastic art in the valley of the Nile. The wheel was not in use; the ware was all hand-made, and sun-dried, or indifferently baked. Two or more varieties of manufacture have been discovered. The material is a breccia of clay and pounded muscle-shell; but, in some instances, the shell has been substituted by small rhomboidal fragments of white spathic carbonate of lime. The ware is of irregular thickness, but exhibits much tenacity, and is capable of resisting irregular dilatation or excessive shrinkage. The ornamentation is plain, neat, sometimes tasteful, but generally rude, consisting of lines, dots, chevrons and zigzag patterns.

The vessels are urns, vases, cups, dishes, etc. The handles of some are tastefully made in imitation of the wolf, fox, etc. A farmer residing near the Great Mound, excavating for an ice-house in a mound, threw out over a dozen handsome

vessels, mostly ornamented with the animal-shaped handles. Many of the mounds contain vessels of the style alluded to. Some of these vessels doubtless, contained viands for the sustenance of the departed; while others were tributes to the worth of the lost, as was customary with many semi-barbaric people. It is a fact worthy of mention in this connection that the oldest specimens of British pottery are principally funereals found in the tombs of the ancient Britains — their barrows and cairns. Some of the finer specimens discovered in the progress of these explorations, show a polishing or glazing, leaving minute striæ, as if done with a tuft of grass dipped into a *barbotti*. Other specimens exhibit handsome and bright coloring.

Without advancing a theory as to the origin of these mounds, or whether the builders were contemporary with those who erected the earthworks in Ohio and to the south-west, I will simply remark, that in all probability, diverse peoples occupied these valleys, and probably these very monuments, in prehistoric times. We have the positive evidence of at least three distinct peoples using and burying in some of these mounds: the actual builders, the Indians and the white man. One of the elliptical mounds in East St. Louis is covered with graves of the early white settlers. In some British barrows, the Pict, the Roman and the Saxon lie entombed in their relative positions. In the mound just alluded to, which is to be removed during the present month, the interesting archæological fact will be exhibited, of three distinct classes of interment in the same *tumulus*.

It was intended to describe more fully the process of exploring these ancient mounds; the modes of construction; the character of their contents and other peculiarities; also to notice the fauna of the mound period, as shown by the mounds and ancient localities, but the time allotted to this paper obliges me to defer these and additional particulars.

These researches afford us a vivid picture of the habits, customs, social and artistic advancement of the prehistoric peoples who occupied the broad valleys and fertile plains of the West. The great carnivora had not yet disappeared from the valley of the Mississippi, and the huge extinct pachyderms were sufficiently familiar to the Aboriginal artist to be faith-

fully depicted. These people subsisted on game, fish, fruits and maize.

Were they one or many? Autochthones, or immigrants? As crania constitute the truest indication of race, it is possible these researches may determine this interesting question of the early races at the West.

That more than one dwelt here, previous to the red man, who can doubt? How many times this great valley and the entire continent has been inhabited and re-peopled, previous to the present, who can even remotely conjecture? The mounds and vestiges of ancient art, clearly indicate different peoples, either cotemporaneously, or at separate epochs before the dawn of history.

We have not reached that advance in science attained by the anthropologists of Great Britain, who pronounce the character of crania from the form of the tumuli in which they occur.

These researches have not yet determined the interesting anthropological fact, if a fact it is, that *long* mounds contain *long* or *dolichocephalic* skulls; and *round* or conical mounds, cover a round-headed, or *brachycephalic* race.

There are, in the extensive and interesting groups on the American Bottom, fine specimens of these separate types, but it is scarcely probable exploration will develope the peculiarities alluded to.

It is proposed to prosecute these researches with energy, during the present season, embracing the entire valley, or, so much of it as is known as the American Bottom. They promise the most valuable results to science. There are a number of mounds in each group, of sufficient importance to demand careful and thorough exploration. The great tumulus of the series, or Monk Mound, should be carefully excavated. But this would require time, patience, and considerable means. Permission has been obtained to make the exploration, but the work is too heavy for individual enterprise. Cannot some measures be adopted by the American Scientific Association for thoroughly prosecuting these researches? The matter is submitted to the wisdom and judgment of the members.

3. REPORT ON ARCHÆOLOGY AND ETHNOLOGY. By W. De Hass, of St. Louis.

THE Chairman of a Special Committee on American Archæology and Ethnology, appointed at the Baltimore meeting, having been prevented by the war, and other circumstances, from presenting the result of the Committees' operation, begs leave, respectfully, to submit the following Report.

The Committee from the first labored under many disadvantages, and serious embarrassment, for want of funds. The anticipated aid from the Government and Societies having failed, the Committee alone relied upon private liberality for means to prosecute the work committed to their charge. A comparatively small fund having been realized from private sources, the system of research which had been adopted was prosecuted with industry and success. The work principally devolved upon the chairman; he laboring without remuneration, and most of the time at his own expense. Notwithstanding these discouragements much has been accomplished. Some of the most interesting localities in the United States have been visited; thousands of earthworks, mounds, circumvallations, etc., examined; specimens of ancient art and industry collected; numerous mounds excavated; extensive groups measured, mapped and described. The result of researches thus far prosecuted have been altogether satisfactory.

The Committees' functions having been suspended during the war, their Chairman can only report up to that period.

Had the Committee not been constantly embarrassed for funds, most valuable and important results would have been secured to science. Even under these disadvantages, it is believed more has been accomplished than all previous efforts to develop the Archæology of the United States.

The Chairman would respectfully represent the rising importance of American Archæology. Its progress is onward, and it is due to the cause of science that some active and energetic measures be taken to promote research among prehistoric monuments in our midst.

Whilst the enlightened Governments of Europe are zealous and active in efforts to trace out the beginning, progress, and

life of man, to develop his remains of art, what being? In Paris, there is a well organized society to the investigation of *American Archæology*. It is creditable to our intelligence as a cultivated people, that so little attention is given this important branch of scientific inquiry.

The chairman would respectfully suggest that some decided action be taken by the Association for promoting inquiry and research. That the functions of the old Committee be either revived, or a new Committee appointed. He is ready and willing to coöperate in any way that may be desired.

He would specially recommend that measures be adopted for securing a National Museum for American Antiquities. To effect this he is willing to make large contributions. He is also willing to devote a portion of his time towards prosecuting scientific research.

He would farther urge another suggestion; that much might be effected by popularizing the subject of American Archæology through public lectures. Those competent to the work should be selected to perform it.

A vast field is open to energy and enterprise, and it is hoped that the importance of the subject will not be overlooked by the friends of science assembled at Chicago.

These views and suggestions are respectfully submitted.

---

4. The New Arctic Continent, or Wrangell's Land, discovered August 14, 1867, by Captain Long, of the American Ship Nile, and seen by Captains Raynor, Bliven and others, with a brief notice of Baron Wrangell's Explorations in 1823. By William W. Wheildon, of Charlestown, Massachusetts.

In December, 1867, considerable interest was excited through the country by the announcement by telegraph from San Francisco, of the discovery of a "New Continent in the Arctic Regions," which had received from the discoverer the appropriate name of "Wrangell's Land," after the distinguished Russian explorer. The discovery was said to have been made

by one of the whale ships visiting the Arctic Regions through Behring Strait, which had just returned to the Sandwich Islands. It was also stated that the season had been remarkably mild — one of the most temperate ever known — and that one of the whale ships had been able to penetrate as far north as latitude 83° 30′. The announcement that land had been discovered in the region indicated seemed probable, but the statement of the latitude reached — never having before been attained in any part of the Arctic regions — was received with some incredulity; in addition to which there was evidently an error in the statement of the longitude as received by the telegraph. To those who were acquainted with the geography of the region indicated, hardly any reported discovery of land would have been incredible, so uncertain and imperfect was the knowledge of it; but it might have been considered, as in the present case, a rediscovery, or perhaps a simple confirmation of a general belief.

When the printed accounts reached us from the Sandwich Islands, it appeared that the land reported was first seen by Captain Long, of the American whale ship *Nile*, of New London, on the 14th of August, 1867, in clear weather. The west point, which he named "Cape Thomas" (after the seaman who first reported the land), was in latitude 70° 46′, longitude 178° 30′ E. On the morning of the 15th, the ship at 9 o'clock, was eighteen miles distant from its western point. "The lower parts of the land," reports Captain Long, "were entirely free from snow and had a green appearance as if covered with vegetation. There was broken ice between the ship and land, but as there were no indications of whales I did not feel justified in endeavoring to work through it and reach the shore, which I think could have been done without much danger."

Captain Long sailed to the eastward, along the land, during the 15th and 16th, and in some places approached it as near as fifteen miles. "On the 16th the weather was very clear and pleasant, and we had a good view of the middle and eastern portions of the land. Near the centre, or about in the longitude of 180°, there is a mountain which has the appearance of an extinct volcano. By approximate measurement I found it to be 2480 feet high."

Captain Long says he had excellent observations on the 16th, and the south-eastern cape which he named Cape Hawaii, he found to be in latitude 70° 40′ north, and longitude 178° 51′ W. He adds, "it is impossible to tell how far this land extends northward, but as far as the eye could reach we could see ranges of mountains until they were lost in the distance." Captain Bliven, of the whale ship *Nautilus*, said he saw the land north-west of Herald Island, as far north as latitude 72°.

Captain Raynor, of whale ship *Reindeer*, in a letter of November 1st, 1867, to the editor of the "Pacific Commercial Advertiser," gives an account of this land, "lying in the midst of the Arctic Ocean, hitherto but little known." He says, "this land has hitherto been considered to be two islands, one of which is marked on the English maps as Plover Island, which is laid down to the W. S. W. of Herald Island. The other is simply marked 'extensive land with high peaks.' On my last cruise [the one just terminated] I sailed along the south and east side of this island for a considerable distance, three different times, and once cruised along the entire shore, and by what I considered reliable observations, made the extreme south-west cape to lie in north latitude 70° 50′ and east longitude 178° 15′. The south-east cape I found to lie in north latitude 71° 10′, and west longitude 176° 40′. The south coast appears to be nearly straight, with high rugged cliffs and entirely barren. The north-east coast I have not examined to any extent, but it appears to run from the south-east cape in a north-westerly direction for about fifteen or twenty miles and then turns to the north and north-east. I learned from Captain Bliven that he traced it much farther north, and has seen others who traced it to north of latitude 72°. I think there is no doubt that it extends much farther to the north, and that there is another island lying to the east of it, say in longitude 170° west, and to the north-east of Point Barrow, with a passage between it and the land I have just described. . . . I would add that the south-west cape of this island described above, lies seventy-five miles distant from the Asiatic or Siberian coast."

According to Captain Long's observations it will be seen that the extent of the discovered land from east to west, is 2°

39′ of longitude, equal to about fifty geographical miles; but according to Captain Raynor's observations its extent is equal to 5° 5′ of longitude, or nearly one hundred and three geographical miles, or about one hundred and twenty statute miles. So also if the statement of Captain Bliven is to be received (and there appears to be no reason why it should not be received) the extent of the land from the south-east cape (Cape Hawaii, of Captain Long) to the north as far as reported by Captain Bliven and others, must be at least one hundred miles and probably more.

The season of 1867, is reported as already stated, to have been remarkably mild. Captain G. H. Soule, of whale ship *St. George*, says "this season is the most remarkable ever known by the whalemen for the scarcity of ice and the good weather prevailing during the first and middle parts of the season. *Otoken*, a very intelligent native of Indian Point, told me they had two months south wind last winter [1866], which I think accounts for the openness of the season." Other captains report "whales very scarce." Captain J. B. Winslow, of the bark *Tammerlane*, says the weather was fine in August and September; whales scarce; took seven; of the third whale he took, he speaks as follows: "The third whale we took was a stunner, and deserves special mention. It was the biggest whale, by at least one quarter, that I ever saw alongside a ship. My third mate kept a tally of what turned out at the cooler, and it yielded *three hundred and ten barrels and nineteen gallons*. It was not so fat as some we caught. I have taken whales that made two hundred and fifty barrels of oil, but never before saw one that would compare in size with this. I think it must have been one of the original whales that Noah had charge of, which has been growing ever since." *

*In regard to the reported scarcity of whales, the following statement will show how the matter stood in 1867, as compared with the previous years:

The number of vessels refitting at the Sandwich Islands in 1867, was	75	
Having sperm oil in barrels,	1940	
" whale " " "	52050	
Averaging to each vessel, in barrels,	720	
Total amount of whale-bone,	11313	lbs.
Number of vessels in 1866,	76	
Average number of barrels to each,	680	
Total amount of whale-bone,	10980	lbs.

Showing an excess of forty barrels of oil to each vessel, in 1867 over 1866, and an excess of whale-bone.

Baron Wrangell, in his explorations along the coast of Siberia, in March, 1823, was visited in camp by a Tschuktschi chief, whom he represents as "a very civilized person in his way," and who drew for him with a piece of burned wood, an outline of the coast in the neighborhood of Cape Schelagskoi. "He farther assured us," says Baron Wrangell, "in the most positive manner, that there was no other island along the coast. When I asked him whether there was any other land to the north beyond the visible horizon, he seemed to reflect a little, and then said that between Cape Schelagskoi and Cape North, there was a part of the coast where, from some cliffs near the mouth of a river, one might in a clear summer's day descry snow-covered mountains at a great distance to the north, but that in winter it was impossible to see so far. He said that formerly herds of reindeer sometimes came across the ice of the sea, probably from thence, but that they had been frightened back by hunters and by wolves; that he had himself once seen a herd returning to the north in this way, in the month of April, and that he had followed them in a sledge drawn by two reindeer for a whole day, until the rugged surface of the ice forced him to desist. His opinion was that these distant mountains were not an island, but an extensive land similar to his own country. He had been told by his father that a Tschuktschi elder had once gone there with a few followers, in large *baidars*, or boats made of skins, but what they found there, or whether they ever returned, he did not know. Still he maintained that the distant northern land was inhabited, and adduced as a proof of it that some years ago a dead whale had been found at Arautan Island [on the coast near by], pierced by spears pointed with slate;* and as the Tschuktschi do not use such weapons, he supposed that the whale must have been wounded by the inhabitants of the northern land." Baron Wrangell's position when he received this information was in latitude 70° 3′ and longitude 171° 3′ East.

A few days after this, Baron Wrangell started with sledges over the ice, with a view to reach or discover this northern land. Following along the coast for a day or two, on the 13th of March, he left the shore and proceeded in a N. N. E. direc-

*The people of the Aleutian Islands use similar spear heads it is stated.

tion. With the usual difficulties of cold, wind, storm and ice-hummocks, he continued on until the 23d, about ten days, having sent his sledges back excepting two. According to his reckoning he reached latitude 70° 51′, longitude 175° 27′. His distance from the coast was one hundred and five wersts or a little more than sixty geographical miles. At the extreme point of his journey, Baron Wrangell says, "we climbed one of the loftiest ice-hills, affording an extensive view towards the north, and from thence we beheld the wide immeasurable ocean spread out before our gaze. It was a fearful and magnificent spectacle. Fragments of ice of enormous size were floating on the surface of the agitated ocean, and were dashed by the waves with awful violence against the edge of the field on the farther side of the channel before us. . . . With a painful feeling of the impossibility of overcoming the obstacles which nature opposed to us, our last hope now vanished of discovering the land which we still believed to exist; and we saw ourselves compelled to renounce the object for which we had striven through three years of hardship, toil and danger. We had done, however, all that duty and honor demanded; and any farther attempts being totally hopeless, I determined to return." At this time being in latitude 70° 51′, longitude 175° 27′ E., Baron Wrangell was less than sixty miles from the westerly point of the discovered land as reported by Captain Raynor.

A few days later, early in April, 1823, Dr. Kyber, companion of Baron Wrangell, became acquainted with some Indian chiefs, who spoke much of a "more northern land, the lofty mountains of which were visible on very clear days from a place which they called Jakan, and which they described with tolerable minuteness." On the 8th of the same month Cape Jakan was reached and found to be in latitude 69° 42′, longitude 176° 23′ E., by reckoning from the previous day. "We gazed long and earnestly on the horizon, in hopes, as the atmosphere was clear, of discerning some appearance of the northern land which the Tschuktschi affirm that they have seen from this place, but we could discover nothing of it." Subsequently M. Matiuschkin made another attempt, with the consent of Baron Wrangell, to reach the northern land over the

ice, with three sledges and provisions for fifteen days. He started from the shore in latitude 69° 28′, longitude 177° 44′ E., on the 9th of April, but succeeded only in getting about sixteen wersts or about eleven miles from the coast, on account of the breaking up of the ice and the many open spaces.

Captain Kellett, of H. B. M. ship *Herald*, in July, 1849, having passed through Behring Strait and standing along the margin of the ice, discovered a group of islands in latitude 71° 20′, longitude 175° 16′ W., one of which he landed upon and named Herald island. Another island which he reported has not since been seen. Captain Raynor, it will be seen, reports another island to the eastward of Herald Island, in latitude 70° W. N. W. from Point Barrow. The Russian navigator, Sergeant Andreyer, it is stated, reached some land off the coast of Asia, in 1762, which he reported to be inhabited by a people named Kraihaï.

SUMMARY.

1. Captain Long's positions, 1867,
   West Point, Cape Thomas, Latitude 70° 46′, Longitude 178° 30′ E.
   South-east Point, Cape Hawaii, Latitude 70° 40′, Longitude 178° 51′ W.
2. Captain Raynor's positions, 1867,
   Extreme south-west Cape, Latitude 70° 50′, Longitude 178° 15′ E.
   South-east Cape, Latitude 71° 10′, Longitude 176° 40′ W.
3. Captain Bliven and others traced the coast northward to Latitude 72°.
4. Baron Wrangell's farthest, in 1823, over the ice, Latitude 70° 51′, Longitude 175° 27 E.
   and did not see the land.
5. Captain Kellett's position, 1849, Latitude 71° 20′, Longitude 175° 16′ W.

Since the preceding pages were written, a new edition of the map entitled "Behring Sea and Arctic Ocean, from surveys of the U. S. North Pacific Surveying Expedition, in 1855, by Commander John Rodgers, U. S. N. commanding, and from

Russian and English authorities," has been issued from the "Hydrographic Office, U. S. Navy," with "additions to July, 1868." The land seen and named by Capt. Long, is laid down upon the map in accordance with his statements; and a drawing is also given of the land, with its elevated points, probably from a sketch made by Capt. Long. "Wrangell's Land," we may here remark, falls about ten degrees westerly of the present boundary of the United States in the latitude of 70°.

The new boundary of the United States passes through the middle of Behring Strait, between the Diomede Islands, one degree south of the Arctic Circle, leaving Ratmanov Island on the Russian side and Kreusenstern Island on the American side. From this point it reaches towards the North Pole on the meridianal line of 168° 50′ west longitude from Greenwich. From the Diomede Islands it runs in a nearly south-west direction in the North Pacific Ocean, to latitude 50°, in longitude about 168° east. So that, at the present time, the territory of the United States extends in an east and west direction from longitude 67° west (at Eastport, Maine), to longitude 168° east in the North Pacific Ocean (beyond the most westerly island of the Aleutian Group), equal to 125° of longitude, which on the parallel of 45° north latitude would be equal to about five thousand three hundred and three geographical miles, or six thousand one hundred and eighty-seven statute miles. The position at the mouth of Columbia River is nearly in the middle of this line and the central point east and west of the United States territory, it being about the same distance from Eastport to the mouth of Columbia River that it is from the Columbia River to the most western extremity of the Aleutian Islands.

Although the newly discovered land does not fall within the enlarged limits of the United States, the honor of the discovery of it belongs to our countrymen; and the act of Captain Long in giving the name of Baron Wrangell to it, although it was never seen by him, is a well-deserved compliment to that intelligent and indefatigable explorer and to his country. The propriety of the name is generally admitted, and it has been promptly, in the first instance, adopted by our government in its first official publication — the map above mentioned — and it will no doubt be universally accepted.

5. THE HABITABLE FEATURES OF THE NORTH AMERICAN CONTINENTAL PLATEAU, NEAR THE LINE OF THE THIRTY-FIFTH PARALLEL OF NORTH LATITUDE. By C. C. Parry, M. D., of Davenport, Iowa.

THE recent rapid progress of settlement in the elevated interior district, properly characterized as the great Western plateau of the North American continent, is suggestive of some general principles of civilized development which may aid us in estimating future progress in particular portions of this district, especially favored by their peculiar location, climate, or natural products, as inviting and retaining a permanent population.

The aboriginal population offers the most direct means of forming a general estimate of the desirable habitable features of particular regions, though these obvious views will necessarily require correction and modification to correspond with the marked differences between the widely distinct classes of civilized and uncivilized life.

Still, other things being equal, the country which, in its ancient monuments, or its present existing races, shows traces of superior condition, large population, or conveniences of living, will necessarily indicate at least an enlarged capacity for the growth and support of progressive modern civilization.

These indices of capacity for sustaining an advanced order of civilization are especially marked near the line of the thirty-fifth parallel, north latitude. This includes in New Mexico and Northern Arizona, the favorite homes of the industrious, semi-civilized, Pueblo Indians. It is also the country especially marked by the scattered remains of an ancient civilization, exhibited in the ruins of populous towns, wisely selected with reference to favorable agricultural districts and broken pottery, showing considerable progress in the domestic arts.

The present condition of the native population, but slightly different from that noted by the early Spanish explorers three hundred and fifty years ago, includes three distinct classes; according to the relative preponderance of each of which, determined in great measure by the external features of the country, its true habitable features is brought clearly to view.

1. There is the roving Apache, with no permanent home, living by theft, and only supplementing this irregular mode of existence by the scanty native product of the soil, or the precarious returns of the chase. Without beasts of burden or domestic animals of any kind, except such as are captured in their predatory excursions and subsequently consumed for food, they roam in scattered bands over a wide section of the country, occupying the rocky recesses and inaccessible canons of the mountain districts, from which, eluding pursuit, they can most successfully prey upon the unsuspecting traveller, or accomplish their thieving raids on the cultivated fields of their industrious native compeers.

2. We have the Navajo tribes, showing an advance towards civilization in the rearing of flocks and herds, upon which they rely mainly for subsistence and clothing. Necessarily connected with this more settled mode of life, we note some considerable progress in the domestic mechanic arts, such as weaving, basket making, etc. Also some attention is given to the cultivation of the soil, and raising fruit to supply the place of the scanty native productions. Still, however, in accordance with their roving propensities, they are addicted to hunting and plunder when it can be safely carried on at points remote from their settled habitations. The Navajo country comprises the picturesque region lying west of the Rio Grande, including an excellent grazing district, and a rocky section with deep canons, clear streams and limited fertile valleys. From this desirable district a large part of the Navajo tribe have been recently removed to a Government reserve, in South-eastern New Mexico, by a very questionable policy, depriving these Indians of their natural means of legitimate subsistence, and supporting them at Government expense in an idleness that can only result in their speedy deterioration, and ultimate extermination.*

3. In the rank of ascending civilization we have the Pueblo Indians, living in defensive towns, relying for subsistence on the cultivation of the soil, and the rearing of domestic animals; industrious, hospitable, fond of trade, ready to adopt any of the improvements in the art of living that can be brought

*According to a more recent military order, the Navajo tribe has been returned to their original locality west of the Rio Grande, where they are making encouraging advances in civilization.

within the scope of their capacity, having an orderly system of government, regulating all their social and business relations, indulging only in defensive warfare, and in every respect exhibiting the most encouraging features of aboriginal civilization.

The Pueblo Indians select, by preference, the larger valleys and fertile districts adapted to cultivation. Their towns are located with a view to defence and observation, and in the interior districts occupy those castellated rocks, corresponding in location and structure to the closely allied remains of the ancient civilization, of which, it is but reasonable to infer, this is but a remote trace.

In this natural parcelling out of a country vast in extent, favored by a most genial climate, and possessing great undeveloped natural resources, we have the means of estimating the character and prospects of an American civilization when transplanted and matured on this elevated central region—"this highway of the nations." Sheltered by high mountain ranges from the sweep of the freezing northern blasts, it presents an open, unobstructed slope to the moist southerly currents, borne respectively from the Mexican and the Californian Gulfs. Hence on the higher districts there is sufficient moisture precipitated to favor the growth of forests, and give origin to perennial streams, which supply the upper sources of the Rio Grande and the Gila. In these elevated valleys the general aspect is that of a northern region, characterized by spring and summer rains, light winter snows, and an elastic, bracing atmosphere. Extensive pine forests cover the higher mountain slopes, interspersed with well watered, grassy valleys, affording a most magnificent summer pasturage. All these manifest advantages are connected with a central geographical position, accessible by ordinary means of travel, and closely adjoining semi-tropical districts, lying to the south. By the navigable waters of the Colorado of the West it has direct water communication with the Californian Gulf and the Pacific Ocean, thus presenting a combination of desirable features that must eventually insure its civilized advancement.

Mineral wealth is an element that does not enter at all into the estimate of productive capacity as derived from a view of its aboriginal population, and this is one of the main qualifying

considerations that must modify our views of a progressive civilization, planting a large population on otherwise forbidding districts. Thus, on the desert regions of Nevada, where a short time since digger Indians eked out a miserable existence on roots, pinons, lizards and grasshoppers, we have seen the development of a single rich mineral vein securing for such a locality a populous district, gathering from abroad all the desirable resources and luxuries pertaining to the most favored localities, in spite of all natural disadvantages. Much more, when such stimulating attractions as mineral wealth draws in its train are added to agreeable natural accessories, may we expect the influx of a prosperous and permanent population, and such will eventually be realized on the now undeveloped gold and silver lodes of New Mexico and Arizona.

The pioneer miner, artisan and agriculturist, are already there, spying out the land, maintaining an exterminating warfare with the relentless and untamable Apache; along the travelled roads and obscure trails, every thicket and rocky covert has echoed with the crack of the hunter's rifle or revolver, and many a canon and rocky slope has been richly watered by pioneer blood. Out of this contest there can be but one solution —the wild Indian must give place to the progressive white man, Anglo Saxon civilization must supplant barbarism, and what the country is capable of being made under the energizing influence of American enterprise it will eventually become.

The experience of the last quarter of a century has abundantly shown that the natural tendency, alike of the popular and scientific mind, is to depreciate and underrate the actual value and natural resources of imperfectly known districts. It is within this period of time that it has been scientifically assumed that beyond the one hundredth degree of west longitude agriculture could not be successfully prosecuted, from the dimin ished quantity of atmospheric precipitation. We have now a practical refutation of this argument in the self sustaining agricultural communities of Colorado, Utah and New Mexico. Only within the last two or three years has it been successfully demonstrated that cereal products of superior quality can be matured, without irrigation, over extensive districts in New Mexico, Arizona and California. In fact, wherever weeds grow

along the road side, there agricultural products, adapted to the particular locality, can be profitably raised. Among the Pueblo Indians of New Mexico irrigation has always been but partially relied on for maturing crops, and with this important accessory carried forward on the enlarged scale of civilized appliances, extensive districts may be redeemed from sterility, and a security given to the returns of agriculture, such as does not pertain to those favored districts that depend solely on the varying contingencies of a regular rainy season. One very manifest advantage of irrigation, as an agricultural process, is realized in the entire security of the crop through the harvesting season, whence results that superior quality of grain, peculiarly adapting it for distant transportation. It is not intended by this strain of remark to assert that there are not in the region under especial consideration extensive desert tracts, and agricultural wastes unsuited for human habitation, but it is safe to say that the extent of these has been grossly exaggerated, and that they are so interspersed with fertile valleys, pastoral uplands, and a variety of natural products, both vegetable and mineral, as to compare most favorably with those Eastern districts whose natural resources are limited, and which are indebted to the old appliances of civilization for most of those advantages which render their particular localities adapted to the requirements of human habitation.

Let the facilities of railroads be once extended to the attractive regions of New Mexico and Arizona, and we shall not be long in witnessing the successful development of vast mining enterprises, the growth of cities, and manufacturing centres, the extension of agriculture over virgin wastes, unexhausted by culture; surplus commercial products, unrivalled in extent, or cheapness, and with the disappearance of the nomadic Indian, we shall soon realize the influx of a permanent population before which the highest attainments of the ancient civilization will hardly afford the means of a faint comparison.

6. On the Leaves of Coniferæ. By Thomas Meehan, of Germantown, Pennsylvania.

Botanists can scarcely have overlooked the fact that the true leaves of *Pinus* consist of bud scales; and that what are known as leaves, and what Dr. Engelman (Gray's Manual, fifth edition, p. 469) calls "secondary leaves," are but phylloid shoots, but I have failed to find any specific reference to the fact in botanical works. Dr. Dickson, however, in a paper on the *Phylloid Shoots of Sciadopitys verticillata* ("Proceedings of Botanical Congress," 1866, p. 124) remarks, "In *Sciadopitys* I have to call attention to the fact that the leaves of the growing shoots consist, as in *Pinus*, entirely of bud scales." One would suppose from this incidental reference to *Pinus* that he was acquainted with the fact that the so-called leaves of *Pinus* were phylloid shoots; but as the object of the paper is to show that the so-called leaves of *Sciadopitys* are not true leaves, and as any one must know that they are not if already cognizant of the fact in *Pinus*, we may take it for granted that at any rate if not entirely overlooked, little thought has been given it. I believe I am occupying an entirely original field in pointing out the true nature of leaves in *Coniferæ*, and that the increased knowledge will have an important bearing on many obscure points in their study.

Dr. Dickson uses but the language of general botany when he describes the true leaves of *Pinus* as "bud scales," meaning thereby the scaly free portion just under the "secondary leaves," of Engelman, and sometimes forming sheaths around them. But these free scales are scarcely leaves. The chief portion of the true leaves in most plants of the order are adnate with the stem. Sometimes they have the power to develop into scaly points, at others into foliaceous tips, and at other times without any power but to preserve their true leaf-like character. *Larix* affords the best illustration. The true leaves are linear spathulate, entirely adnate to the stem. There are two kinds of stem growth on *Larix*. In the one case the axis elongates and forms shoots; in the other axial development is arrested and spurs are formed. On the elongated shoots the leaves are scattered; on the spurs they are arranged

in whorls. The power of elongation possessed by the shoot is imparted to the leaves which are adherent to it, and they produce green foliaceous awl-like tips; the power of elongation which the spurs have lost is also measurably lost to their leaves, they develop themselves fully, although they have no stem to adhere to; they preserve their spathulate form, but cannot produce the awl-shaped tips of the shoot leaves. There are, therefore, two forms of leaves on the larch, the one free, the other adherent; and we have a novel principle very clearly illustrated that *strong axial development* (vigor) *is a characteristic of adhesion, while the reverse* (weakness) *is characterized by a free system of foliation.* Any species of *Larix* will sustain this observation; and, *leptolepis*, as a vigorous grower, is the best.

The characteristics of the foliage described in *Larix* may be found in a greater or less degree in a great many species of coniferous plants. In *Cryptomeria* the leaves adhere for four-fifths of their length on vigorous shoots; but on the more delicate ones they are free for three-fourths or more. In *Juniperus* the different forms of foliage are well known, especially in *J. Virginiana*, *J. Chinensis*, and *J. communis*. On the vigorous shoots adhesion takes place for nearly the full length of the leaves, but on weaker ones the leaves are very nearly free. In *Thuja*, *Biota*, *Retinispora*, *Cupressus*, *Thujopsis*, indeed most of the Section *Cupressineæ* these variable degrees of adhesion may be found, and always in relation to the absence or presence of vigor; and on this question of vigor it will be well here to make a few remarks. The power to branch I take to be a high mark of vigor. The young seedlings of most coniferous trees grow but a few inches the first year and have no power to branch. The power increases with age, and in all cases in proportion to the vigor of the plants. In *Thuja*, for instance, no branches appear till the second year. They increase in number, until, when in its prime, branches appear from every alternate pair of axils, and, as these are decussate, this gives the fan-like form of growth of which the Arbor Vitæ affords a familiar illustration.

This varying power of adhesion in the true leaves, and in connection with vigor enables us to explain many matters

hitherto not understood. For instance, Dr. Lindley describes a form of *Biota* as *B. Meldensis*, suggesting that from its appearance it must be a *hybrid between the Red Cedar and Chinese Arbor Vitæ*. It is but *B. orientalis* with the leaves moderately united. *Thuja ericoides* of gardens was long supposed to be a Japanese species; it is but an entirely free-leaved form of *Thuja occidentalis*. *Retinispora ericoides* of Zuccarini is but a free-leaved form of some Japanese plant; and in all probability many species of *Retinispora* so marked in herbariums, are all forms of one thing with more or less adnated leaves. In all these cases delicacy of growth and freedom of leaves go gradually together as before indicated.

One of the most remarkable instances of the value of this principle however, will, I have no doubt, be in fixing the identity of the Japanese genus *Glyptostrobus* * of Endlicher, with the American *Taxodium* of Richard. In a shoot one foot in length of the latter we find perhaps four or six branchlets; in the same space in *Glyptostrobus* we shall find a score or more. Indeed, in this plant, a branchlet springs from nearly every axil on the main branch, showing an extraordinary vigor. As vigor is opposed to a free development of foliage, the small thread-like leaves of *Glyptostrobus* are naturally to be expected, and the free leaves distichously arranged is the natural concomitant of the weaker *Taxodium*. Fortunately I am able to sustain this theory by actual facts. I have a seedling tree ten years old of remarkable vigor. It does not branch quite as much as the typical *Glyptostrobus*, but much more freely than any *Taxodium*. The result is the foliage is aciculate, not distichous, and just intermediate between the two supposed genera. But to help me still more, my tree of *Glyptostrobus* has pushed forth some weak shoots with foliage identical in every respect with the intermediate *Taxodium*. Specimens of all these are presented with this. In establishing *Glyptostrobus*, Endlicher notes some trifling differences in the Scales of the Cones between it and *Taxodium*, but all familiar with numerous individuals of some species of *Coniferæ*, *Biota orientalis* for instance, know how these vary. There can be no doubt I think,

* *Note by the proof reader*, — It was the intention of the author to refer his remarks on Glyptostrobus to *G. Sinensis* Endl.

of the identity of the two; and this will form another very interesting link in the chain of evidence, that the flora of Japan is closely allied to that of the United States.

If we were to look on the so-called leaves of *Pinus* and *Sciadopitys* as true leaves, we should find serious opposition to my theory that a vigorous axial growth is opposed to the development of free leaves in *Coniferæ*, for we should see a class of plants which notoriously adds but from three to six branches annually to each axis, clothed with foliage. But admitting them to be phylloid shoots, it confirms our theory in a strong degree. We then see a plant loaded with branchlets, and so great is the tendency to use them instead of leaves, that in some cases as in *Pinus strobus*, *P. excelsa*, and others of a softer class of *Phylloideæ*, the bud scales are almost entirely confined to the sheathing leaflets; just as in the very rugged, hard leaved, almost spinescent forms, like *Pinus Austriaca*, we find them more dependent on well developed adnate leaf scales. In *Abies* of old authors, *A. excelsa* for instance, we have a numerous branching tendency; hence we have true leaves, though partially adnate, and no necessity for phylloid branchlets. In *Picea* of Link, almost near *Abies*, taking *P. Balsamea* as a type, we have a rather weaker development, slower growing and less hardy trees, and the leaves are nearly free. Could some of the shoots of *Abies* be arrested in their axial development as in *Larix*, we should have the remainder increased in length, and the fewer branchlets, and two forms of leaves just as in *Larix*. Should, on the other hand, the plant increase in vigor there would be no class of free leaves, adnation would be the law, and metamorphosed branchlets prevail. Starting from *Abies* extra vigor makes the Pine; extra delicacy the Larch. It is the centre of two extremes.

That the fascicles in *Pinus* are phylloid shoots I think cannot be questioned. Their position in the axils of the true leaves, as beautifully shown in *Pinus Austriaca*, indicates the probability. Their permanency in proportion to their induracy is also another strong point. The soft ones of the Strobus Section retain vitality little longer than some true leaves, while the spinescent ones of *P. Austrica* remain green for four or five years. But the strongest of all points is that on dis-

section of an old fascicle it will be found to have a distinct connection with the woody system of the tree, and that these minute woody axillæ under each fascicle, increase in size with the age of the sheath. With a very little encouragement these woody axillæ can be induced to elongate and become real branchlets. If the leading shoot, for instance, of a Pine be tipped in May just after pushing, bulblets will form in every fascicle below, and the next season, the bulblets will produce weak branchlets, although this might be said of true leaves, which are supposed to bear an embryo shoot in the axil of every one. So much stress need not be put on this fact as the others are sufficient. It is introduced and its weak nature commented on, as it furnishes the chief point in Dr. Dickson's argument for *Sciadopitys*, which amounts to little more than that the apparently single Phylla is really a double one—a two-leaved fascicle united by a transformed sheath through its whole length. Carriere has since pushed Dr. Dickson's observations farther by noting in the "Reveu Horticole" really bifid leaves, with little verticils in the axils (see reference in "Gardener's Chronicle," May 2, 1868), an observation which I confirm by a specimen exhibited herewith; yet as I have said it is by itself not wholly free from the objection that it may be but a modified form of regular bud growth; but together with my other observations I think they do serve to confirm the point of these so called leaves being but phylloidæ.

In conclusion I will restate the main points of this paper:

The true leaves of Coniferæ are usually adnate with the branches.

Adnation is in proportion to vigor in the genus, species, or in the individuals of the same species, or branches of the same individual.

Many so called distinct species of coniferæ are the same, but in various states of adnation.

7. Description of a New Species of Protichnites, from the Potsdam Sandstone of New York. By Professor O. C. Marsh, of New Haven, Conn.

The first discovery of footprints in the lower Silurian of this country, appears to have been made in 1847, in the Potsdam sandstone of Beauharnois, Canada East. In 1851 an account of the locality was published by Sir William E. Logan, and with this appeared a short description of the impressions themselves by Professor Owen, of London, who then considered them the tracks of a Tortoise.* Subsequent explorations by the Geological Survey of Canada brought to light new localities and additional specimens in the same region, and in the following year the geological age of the strata containing them was fully discussed by Sir W. E. Logan, and a more detailed description of the footprints was given by Professor Owen, who now regarded them as made by a large Crustacean, probably allied to the modern *Limulus*.† He applied to these impressions the generic name *Protichnites*, and distinguished six species, or varieties, apparently quite distinct from each other. A trail of a different character was afterwards discovered in the same formation near Perth, Canada West, and described by Sir W. E. Logan under the name *Climactichnites Wilsoni*.‡ This was supposed to be the track of a Gasteropod, but Professor Dana has since suggested that it may have been made by a large Trilobite.§ Up to the present time, eight localities of footprints have been found in the Potsdam sandstone of Canada, along the strike of the formation, for about four hundred miles, and all at nearly the same horizon, or within fifty to seventy feet of the top.‖

From the lower Silurian of the United States, no footprints appear to have been described hitherto, although their existence in these strata is now clearly established. An interesting series of impressions was discovered by the author on the western shore of Lake Champlain during a visit to that region

*Journal Geological Society of London, vii, pp. 247 and 250.
†Journal Geological Society of London, viii, pp. 199 and 214.
‡Canadian Naturalist, v, p. 279, 1860.
Manual of Geology, p. 189.
Geology of Canada, p. 108, 1863.

immediately after the meeting of the Association at Burlington in August last. They were found on a ripple-marked surface of Potsdam sandstone, just above the then water-line of the lake, a short distance north of the village of Port Kent. The rock was a hard white quartzite, resembling that containing the footprints in Canada, although probably belonging to a somewhat lower horizon. The impressions obtained, which indicate a new form of Crustacean track, were in two portions on the same surface, and were evidently made by the same animal. They form together a series of footprints about six feet in length, consisting of two parallel rows of impressions, separated from each other by a space of about one and three-fourths inches, and having an extreme width between their outer edges of two and one half inches.

One of the most striking features of this series, which readily distinguishes it from the *Protichnites* already described, is the absence of a medial trail or tail-mark. No indications of such an impression could be detected, even when the track passes over the ridges of the ripple-marks, where it certainly would appear, if at all. The footprints indicate, moreover, a much smaller animal than those that made the other forms of *Protichnites*, the width of the series being in general less than one-half that of the other species, excepting *P. multinotalus*, from which, however, it differs in the absence of the medial groove, and in the less numerous and more irregular foot-marks.

The margins of the individual impressions are not, in general, accurately defined, partly, perhaps, owing to the fact that the surface of the sandstone had been somewhat worn by the water and ice of the lake. In this way some of the smaller impressions may have been obscured or obliterated, as it is now difficult to trace with certainty, through the whole series, the successive groups of footprints which correspond to the repeated applications of the same limbs of the animal, although such a repetition is easily recognized where the tracks are best preserved. In such places the impressions are seen to be arranged in groups of six, which in each row evidently represent successive applications of the same series of limbs, the corresponding set on the other side making a group nearly or

quite opposite when the animal moved directly forward, but partially alternating when it turned to the right or left. Each group appears to consist of four impressions in the linear series, and two additional ones inside and a little behind the last two. The divergence of the impressions thus produced probably indicates, as Professor Owen has shown in regard to the other species, the direction in which the animal was moving. The bottom of some of the tracks are sharply defined, and were evidently impressed by some hard pointed limb. This would tend to show that they were not made by a Trilobite, to which their origin might otherwise naturally be attributed, while the absence of a medial groove would probably exclude a Crustacean of the type that made the other *Protichnites*. The points of difference between this form and those hitherto described are sufficiently marked to justify its separation, and it may appropriately be named *Protichnites Logananus*, in honor of Sir W. E. Logan, Director of the Canadian Geological Survey, to whom this department of palæontology is so much indebted.

The peculiar interest attached to these various footprints on the oldest Silurian strata depends in part upon the fact that we thus have evidence of forms of life before unsuspected; since no other indications of the animals that made the impressions have as yet been discovered. An exception should perhaps be made of the small Limuloid Crustacean (*Aglaspis Barrandi*) described by Professor Hall, from the Potsdam sandstone of Wisconsin and Minnesota, which may have made footprints somewhat similar to the *Protichnites*.* Indistinct impressions, resembling those from Canada have indeed been observed by Mr. Daniels in that formation in Wisconsin, although not at the same horizon in which the remains were found. The *Aglaspis* of the Potsdam and the *Eurypterus* of the Upper Silurian at least indicate the general affinities of the Crustaceans that have recorded their existence in the *Protichnites*, and the remains of the animals themselves will, doubtless, be brought to light at no distant day, and thus reveal their true nature.

* Sixteenth Regents Report on N. Y. Cabinet, p. 181, 1863.

8. On the Preservation of Color in Fossils from Palæozoic Formations. By Professor O. C. Marsh, of New Haven, Conn.

(ABSTRACT.)

The fossil shells in the more recent formations, especially those of Europe, have not unfrequently been found to retain their original color-markings more or less perfectly; but specimens in this state of preservation have very rarely been detected hitherto in the older rocks. The few instances observed, however, are of especial interest to geologists, since the investigations of Professor E. Forbes and others have shown that, in the existing seas which have been explored, shells with well defined color patterns are in general only found in comparatively shallow waters, and the same species may be highly colored when near the shore, and white when inhabiting depths below one hundred fathoms. This was probably true also of the Mollusca in the palæozoic seas, and we may thus have some indication of the depths in which they lived.

The first specimen with the color distinctly preserved, which appears to have been noticed in European palæozoic strata, was a *Pleurotomaria*, from the subcarboniferous limestone of England, described in 1836 by Professor Phillips as *P. flammigera*.* Subsequently one or two other species from the same formation in Ireland were found to be similarly marked, especially the common *Terebratula hastata* Sowerby, and from this fact Professor Forbes inferred that the limestone containing them was deposited in an ocean not exceeding fifty fathoms in depth — a conclusion, however, somewhat at variance with the results of more recent explorations.†

Probably the most interesting fossil, with color markings, from the palæozoic yet discovered is a specimen of *Orthoceras*, found in the Devonian at Paffrath on the Rhine, and described in 1842 by M. d'Archiac and de Verneuil as *Orthoceratites anguliferus*.‡ This specimen, which is now in the private collection of M. de Verneuil in Paris, is still covered with perfectly

* Geology of Yorkshire. Part II, p. 226.

† See paper by M. Sars. Videnkabs-Selskabs Forhandlinger, pp. 246-275, 1868; and Annals and Magazine of Natural History, p. 423, 1869. Also Count Pourtale's Contributions in Bulletin of Museum of Comp. Zöology, p. 126, 1868.

‡ Transactions of the Geological Society of London, 2 series, vol. i, p. 346. 1842.

defined zigzag bands of the original color. While in Germany in 1865, the author was fortunate enough to procure from the same locality a second specimen of this species, with similar bands of color, and these two are probably the only specimens yet discovered. Several gasteropods (*Natica subcostata* Schl.), with distinct color markings, were obtained at this locality at the same time, and others have occasionally been found there.

These discoveries in European strata led the author to examine the palæozoic fossils of this country, where the subject appears to have attracted but little attention as yet, and he had already found indications of color in specimens from several formations. The most interesting of these were Cephalopods from the Trenton limestone of New York, especially *Endoceras proteiforme* Hall, which in several instances he had found to retain distinct traces of the original color, arranged in delicate cancellated patterns. In other species from this formation, and particularly in some *Orthocerata* from the Hudson and Niagara limestones of Illinois and Iowa, he had also noticed indications of color markings. The author exhibited a series of fossils from the palæozoic rocks of Europe and this country, all showing indications of the original color, and stated that a careful examination of our palæozoic strata, especially the light-colored limestones and shales of the West, would undoubtedly bring to light many additional specimens, and thus afford new evidence of the condition of the seas in which they were deposited.

---

9. ANNOUNCEMENT OF THE EXISTENCE OF CRETACEOUS ROCKS IN GUTHRIE COUNTY, IOWA. By C. A. White, M. D., of Iowa City.

IN July, 1867, I announced in the American Journal of Science and Arts, the discovery of the sandstones of the Dakota group of Cretaceous rocks as far south in Iowa as Red Oak Junction, Montgomery county, and within thirty miles of the northern line of the State of Missouri.

I have now to announce the discovery of the same rock in Guthrie county; one locality being within less than forty miles

of the city of Des Moines, and more than eighty miles eastward from the Missouri River.

Further investigations concerning its distribution are now in progress, which it is believed will result in showing a much greater easterly extension of that formation than it has been hitherto believed to possess.

---

10. On the Geology of Lower Louisiana and the Rock Salt Deposit of Petite Anse. By Eugene W. Hilgard, Ph. D., of Oxford, Mississippi.

(ABSTRACT.)

The discovery in 1862, of a deposit of rock-salt on the coast of Louisiana, was a fact so unexpected to geologists, that at any other time a detailed investigation of its geological relations would quickly have followed the first announcement. The pressing necessities of the blockaded section soon caused its development on a large scale, though in a very irregular manner; for a considerable period, these mines supplied the whole of the south-west. In November, 1865, Professor Richard Owen made a brief examination of the locality, the results of which he published in the Transactions of the St. Louis Academy. A year later Dr. Charles A. Gössmann, under the auspices of the American Bureau of Mines, made an examination of the locality, mainly with a view to the working of the deposit. His report, published by the Bureau, as well as the specimens which he courteously exhibited to me, confirm previous conjectures that the overlying strata were the equivalents of the formation I have described as the "Orange Sand" of Mississippi. I therefore gladly availed myself, at the earliest possible moment, of the offer of the Smithsonian Institution to defray my expenses in making a detailed geological investigation of the region. The low stage of water prevailing at the time (December, 1867) rendered it possible to observe, to the best advantage, the formations exhibited on the banks of the Mississippi; the examination of which, from Vicksburg to the Passes, was a needful preliminary step to the determination of the formations of the coast.

Having previously examined and described the sections exhibited at Vicksburg, Grand Gulf and Fort Adams,* I merely landed at some intermediate points to verify the conclusion previously reached, viz., that below Vicksburg no marine formation crops out on the river banks, reports to the contrary notwithstanding; and that the profiles at Natchez, Rodney and other points are essentially similar to that at Fort Adams, where we find the strata of the (fresh-water) "Grand Gulf group" in a position nearly or quite horizontal; overlaid, first by the materials of the "Orange Sand," which in its turn is capped by the stratum of the "Loess" or Bluff formation, covered by a thin deposit of "Yellow Loam."

Facing southward from the "Blockhouse hill" at Fort Adams, we observe a wilderness of the characteristic sharp ridges of the Loess region, often fore-shortened into veritable peaks, elevated between 300 and 400 feet above the river. In this region the Grand Gulf strata have been traced southward by Dr. George Little, the present State geologist of Mississippi, as far as the head-waters of Thompson's Creek, northwest of Clinton, La.

The Orange Sand proper is visible, near the river, as far south as Jackson, La., but farther inland extends to a lower latitude. As for the Loess, it appears in full force and characteristically developed for some distance south of Fort Adams. But (according to Dr. Little's observations) these features become gradually modified as we advance southward. The Loess deposit thins out, its materials become poorer in lime and fossils, and assume more and more the character of a common fine grained "hardpan"; the transition being by insensible degrees, while the two extremes are very obviously distinct. At the same time the clayey substrata which, farther above appear only in patches (as at Nevitt's bluff, two miles above Natchez, as well as at the latter place itself) are seen more frequently and continuously, until, at Port Hudson, they become predominant.

The exposure at Port Hudson, previously examined in part by Bartram, Carpenter and Lyell, is about three miles in extent, from the mouth of Sandy Creek above the town, to Fon-

* See Report on the Geology and Agriculture of Miss., 1860.

tania Landing 1½ miles below. Its lower half is washed, and continually encroached upon, by the river; its upper portion is now inland of an extensive sand-bar. The strata are disposed horizontally or basin fashion, and vary a good deal both in thickness and materials, as shown in the subjoined profiles, situated about a mile apart; the correspondence of strata is ascertained by actual tracing of the stratification lines.

Near Sawmill, Port Hudson.	No.	*Midway betw'n Port Hudson & Fontania.*
Yellow surface loam, 4—6 ft.	6	Yellow loam, sandy below; 8—10 ft.
Yellow hardpan, ... 25 ft.	5	White and yellow hardpan, ... 18 ft. . . . . . . Orange and yellow sand, sometimes ferruginous sandstone, irregularly stratified, ... 8—15 ft.
Heavy greenish clay, ... 7 ft.	4	Heavy, greenish or bluish clay, . . 7 ft.
Gravel, sand and clay in irregular bands, like river alluvium; with pebbles, driftwood, leaves, and Mastodon bones, ... 6 ft.	3	White indurate silt or hardpan, 18 ft.
Heavy, greenish or bluish, massy clay, similar to No. 4; 25 ft. visible.	2	Heavy green clay, with porous calcareous concretions above, ferruginous ones below; some sticks and impressions of leaves, ... 30 ft.
	1	Brown muck, ... } with cypress White or blue clay, } stumps, 3—4 ft.

At the stage of extreme low water prevailing at the time, the stump stratum No. 1 was visible to the thickness of 10 feet at its highest point; showing several generations of stumps above one another, also the remnants of many successive falls of leaves and overflows. The wood is in a good state of preservation; no prostrate trunks to be seen at present.

The main clay deposit, No. 2, varies but little in general character; although very solid, its tendency to cleave into prismatic forms renders it very liable to "cave" into the river. The upper portion of the stratum, especially near its southern end, contains strings of calcareous nodules, on stratification lines eight to twelve inches apart. No fossils save rare impressions of leaves.

No. 3 is exceedingly variable. At the northern end of the outcrop, it is a narrow band of swamp deposit; at the first of the profiles given, it bears the character of a sand-bar; lower down it returns to that of a swamp deposit; still below it is represented by a fine white silt, without a trace of vegetable remains. Lower down again a lignitic layer appears at its

base, with leaves and fruit of living species of lowland trees; while near Fontania it is again a sand-bar with an abundance of prostrate trunks of drift-wood, coarse sand and pebbles.

The green clay stratum No. 4 varies little, either in thickness or composition, and like the stump stratum, No. 1, forms a convenient level of reference.

The hardpan stratum No. 5 I conceive to be the more immediate representative of the Loess proper, with which it is connected by gradual transition, though at times greatly resembling some of the materials of the Orange Sand. It is void of fossils.

The present profile differs in many respects from those given by previous observers, which lay some distance farther west, where the river now flows. The strata are accordingly as variable in an east and west as in a north and south direction, and with the exception of Nos. 1 and 2, are such as are now shown in ditches cut into the modern river-bottom deposits.

The stump stratum, No. 1, however, as appears from numerous data collected by myself or contained in Humphrey's and Abbot's Report on the Mississippi River, exists at about the same level (*i. e.*, near that of tide-water), not only over all the so called Delta-plain of the Mississippi, but also higher up, perhaps as far as Memphis, and all along the gulf coast, at least from Mobile on the east to the Sabine River. Wherever circumstances allow, the overlying clay stratum, No. 2, is also observed. These facts indicate the wide spread prevalence, during the epoch succeeding the drift, of quiet, shallow fresh-water lagoons and swamps of slightly varying elevation; through which the continental waters may for some time have found an outlet, without a definite channel, representing the Mississippi of to-day. The Port Hudson profile appears to be typical, its features being reproduced wherever denudation has not removed these deposits down to the level of the stump stratum, as is mostly the case.

THE FIVE ISLANDS.

The chain of five islands rising partly from the sea, partly from the coast marsh, between the mouth of the Atchafalaya and Vermilion River, have been described by Mr. Thomassy,*

* Géologie pratique de la Louisiane; New Orleans, 1860.

who attributes their origin to "hydrothermal" or "volcanic" action. His descriptions are sufficiently faithful to show the general resemblance of their geological structure; so that after visiting the three middle members of the chain, viz.: Cote Blanche, Week's Island, and Petite Anse, I have thought it superfluous to extend my examination to the two extreme ones, viz.: Belle Isle, the promontory west of Atchafalaya Bay, and Miller's Island (or "Orange Grove") overlooking the plains of the Vermilion. These elevations lie nearly in a straight line bearing N. W. by W. from Belle Island.

COTE BLANCHE.

The next in order, affords on its sea-face a fine exposure of the lower members of the Port Hudson profile. At tide-level we have the blue clay with cypress stumps, the tops of which are often surrounded by alternate layers of clay, muck, and sometimes lignite. The overlying strata consist partly of blue clay similar to No. 2 at Port Hudson, partly of various colored loams alternating with the former; and exhibiting the same calcareous or ferrugino-calcareous concretions along the stratification lines. At a few points these calcareous concretions resolve themselves into distinct fossils, representing the fresh-water genera *Paludina*, *Melania*, *Unio*, *Anodonta* and *Cyclas*, in an indifferent state of preservation. The entire visible profile is about fifty feet high; the highest point of the island rises as high as 180 feet, but in its interior no exposures exist, so that the higher members of the series are not verifiable.

WEEK'S ISLAND.

This island, lying 6 miles N. W. by W. from Cote Blanche, has an area slightly greater, viz., 2,300 acres; it is nearly circular; maximum elevation 160 feet above tide water. Unlike Cote Blanche it is traversed by deep ravines which exhibit the geological structure. In the central and highest portion these gullies are bordered by steep slopes composed of the most characteristic materials of the Orange Sand group. On the exterior slopes, however, we find in a position inclined away from the centre of the island, the lower strata of the Port

Hudson profile—green or blue clay with calcareous concretions, and imperfect fresh-water shells. The blue clay stratum with cypress stumps is met with in ditching, and is also known to exist in the beds of the neighboring bayous, as well as in the surrounding marsh.

PETITE ANSE, OR AVERY'S ISLAND.

Petite Anse lies about 12 miles N. W. by W. of Week's Island, and in its general structure much resembles the latter, to which it is slightly inferior in size, and about equal in elevation; its highest point, "Prospect Hill," on the north side, being 160 feet above tide-level.

An elevated ridge connects Prospect Hill with another high point near the southern slope of the island; and near the west end a ridge, on which Judge Avery's house stands, falls off steeply toward the Bayou Petite Anse. These three points inclose the valley in which the salt deposit has been found and which opens south-eastward into the marsh.

The topography of the island, as well as the history of the mine, have been ably given by Dr. Charles A. Gössmann of Syracuse, in a report of the American Bureau of Mines.* Up to the time of his visit all the pits and shafts had been sunk through detrital strata, washed down from the adjoining hills, and frequently inclosing the vestiges of both animal and human visits to the spot. Mastodon, buffalo and other bones; Indian hatchets, arrowheads and rush baskets, but above all an astonishing quantity of pottery fragments, have been extracted from the pits. The pots doubtless subserved the purpose of salt-boiling; human handiwork has, however, been found so close to the surface of the salt, as to render it likely that its existence in mass was once known, before the time when, in 1862, Mr. D. H. Avery struck the salt itself at the bottom of a salt water well.

The surface of the salt undulates considerably, so that borings commenced at different levels have repeatedly struck salt at nearly the same relative depth, the absolute level of the rock-salt surface varying from 32 feet below to $1\frac{3}{4}$ feet above tide-level. The salt stratum has itself been penetrated to the

* On the rock-salt deposit of Petite Anse, New York, 1867.

depth of 38 feet, without any perceptible variation in quality; its "floor" being as yet unknown. Dr. Gössmann's observations and specimens proved to his and my satisfaction the existence of the Orange Sand on the island; but its relation to the rock-salt, and the age of the latter, remained undetermined.

Since then, another shaft has been sunk by Mr. Chouteau of St. Louis, with the assistance of Mr. Dudley Avery, to whom I am indebted for a record of the strata penetrated. This shaft was located at a higher level than any previously sunk, on a hillside where, not far off, the Orange Sand crops out *in situ*. After passing through these strata the rock-salt was struck again, at a level several feet higher than on any former occasion.

There can therefore be no doubt that the salt deposit is older than the Orange Sand, which here, as at Weeks's Island, forms the nucleus of the mass on whose outer slope, as well as its higher points, the strata of the Port Hudson profile reappear characteristically; with calcareous nodules, fresh-water shells and aquatic plants identical with living species. Not only is the reference level of the cypress stump stratum the same as elsewhere, but the green clay band, No. 4 of the Port Hudson profile, is also there.

The stratigraphical disposition of these deposits was quite remarkable. They conform not to the *strata*, but measurably to the *outline* of the Orange Sand nucleus, roughly following its slopes and curvatures. At first sight therefore it seems as though a local upheaval had taken place, and hence arose, probably, the reports attributing a volcanic origin to these elevations, whose isolated position in the level coast region would naturally give rise to speculation as to their mode of formation. Indeed the extent to which these strata are sometimes seen to dip rather staggers the observer; but the upheaval hypothesis does not explain the facts, unless we are content to assume a separate effort of the sort for every hillock on the islands.

There can be no doubt that subsidence subsequent to deposition has been the cause of the extravagant dips observed sometimes. Where the Port Hudson series is more immediately superimposed upon the Orange Sand nucleus the dips

are moderate, and such as may well be assumed as resulting from deposition on inclined surfaces. But when we see an apparently undisturbed clay-stratum moving down hill like a glacier, so as to overthrow a deposit of loose stones, we need not go far to find the cause of extensive dislocation and subsidence.

BELLE ISLE AND MILLER'S ISLAND.

All the data I have been able to collect concerning the structure of these exterior islands tend to confirm the probable supposition that, like the three interior ones, they consist of denuded nuclei of Orange Sand materials, upon which the Port Hudson series was afterwards deposited.

It seems likely that the same is true of a low ridge called Cote Gelée, in Lafayette parish, bearing N. or N. N. E. Thomassy places in the same category the Grand Coteau des Opelousas and the Avoyelles prairie.

AGE OF THE SALT DEPOSIT.

The Orange Sand strata so rarely approach the coast that the deposits underlying them in the Coast region have scarcely been observed with certainty. Even the older strata underlying the blue stump clay have been observed at a few points only, viz.: by the Delta Survey in the bed of the Mississippi river at Bonnet Carré and Carrollton, near New Orleans; at the latter city itself, in the boring of wells; at Salt Point, on Bayou Sallé; and on the coast of Mississippi Sound.

The strata penetrated in the borings at New Orleans are considered by Sir Charles Lyell as Delta Deposits. But according to my examination they are almost throughout demonstrably of marine origin, and while the species they contain are mostly (not all) now known to be living on the Gulf coast yet the prevalence of species is very different from that now observed near the mouths of the Mississippi. In this respect the fauna of these strata shows a great analogy to those described as Pliocene by Tuomey and Holmes, occurring on the Carolina coast.

It is most probable that the rock-salt of Petite Anse will be found when pierced, to be imbedded in the equivalents of the

deposits penetrated at New Orleans and Bayou Sallé, and of corresponding, probably early quaternary, age anterior to the drift or its southern representative, the Orange Sand.

ORIGIN AND EXTENT OF THE SALT DEPOSIT.

The absence of layers of the usual impurities of rock-salt, especially of gypsum, has induced Dr. Gössmann to suppose that it is not the result of the evaporation of sea-water, but owes its formation to crystallization from the purer brine of salt springs.

Our knowledge of the facts is still too limited to render a discussion of this point very profitable. In a very deep lagoon, withdrawn from the influx of the tides after the brine had acquired a considerable degree of concentration, all the gypsum might be found in a single bed at the bottom; upon it a large mass of pure salt, as in the present case; while the salts of the mother waters would naturally have been washed away from the top. Or there might have been a succession of lagoons communicating with each only during high tides, and acting in a manner analogous to the process now practiced in salt-making on the sea-shore. The gypsum would then all have been deposited in the outer lagoons, while the inner ones would have acted as brine-pits, where pure salt alone could crystallize. Crystals of gypsum have repeatedly been found in shallow wells on the coast beneath the "stump clay."

Upon any of the foregoing suppositions, calling into play a variety of circumstances not likely to be all simultaneously fulfilled, it does not seem probable that the rock-salt mass is very extensive horizontally, or that such masses should occur frequently in the coast region.

A mass of salt 144 acres in extent and 38 feet thick is, however, a handsome specimen, even if these dimensions should represent maxima. The great difficulty in mining it, heretofore, has been the influx of water through the gravelly strata overlying. But it has most probably been attacked, thus far, at its lowest surface level. Wherever elsewhere the Orange Sand formation prevails, it rests on a deeply denuded surface; and "hills within hills" are of very common occurrence. From the data thus far obtained it appears that the same is

the case with the rock-salt mass, and that its surface roughly conforms to the hills and valleys now existing. Workings should be begun at higher levels; and it would not surprise me to learn that the auger had shown the mass to be accessible by level adits in lieu of shafts, on the hillsides. The interior of the solid mass once gained from a point secure from surface water, all difficulty would be at an end.

GEOLOGICAL HISTORY OF THE LOWER MISSISSIPPI VALLEY.

It appears from the facts stated in the preceding pages, that after the termination of the epoch of that Eocene period, represented by the Vicksburg group of fossils, down to the Quaternary era, marine deposits ceased to be formed on the northern border of the basin now represented by the Gulf of Mexico.

I have acquired the certainty of the existence over a large portion of northern Louisiana of the "Grand Gulf" series of rocks. From specimens in the collection of the New Orleans Academy of Sciences, it appears that, apart from the usual materials forming these beds in Mississippi, they assume in the Harrisonburg region the character of compact limestone, which in places is said to be fossiliferous, and would thus furnish the clue to the age of the Grand Gulf group for which I have vainly sought in Mississippi. The problem is one of great interest, as it involves the question whether or not the Mexican gulf has, within comparatively modern times, been disconnected from the Atlantic Ocean. The absence of the cauldron in which the Gulf Stream is concocted might have exerted climatic influences reaching beyond the American continent, and would explain many discrepancies between ancient and modern faunas on the shores of the Atlantic.

It appears that similar limestones, almost assuming the character of black marble, occur in St. Landry parish, near Opelousas. Whether the southern outline of the formation passes thence toward the Calcasieu region, where petroleum has been found, or whether it trends north-westward into the parishes of Sabine and Natchitoches, where limestone and sandstone ridges also exist, is a question still open. In the latter case, this outline would conform to the general shore lines of the great cretaceous and tertiary Mediterranean.

In Mississippi the Grand Gulf series is mostly overlaid by the Orange Sand, deposited on a deeply eroded surface, and bearing itself the evidence of its formation by fresh water in a state of violent flow.* The southern outline of the main body of the Orange Sand runs southward of Opelousas, toward the mouth of the Sabine, whence, according to reliable information, a broad band of shingle extends toward Harrisonburg, Catahoula parish. This belt represents, probably, the most westerly bayou of the great Orange Sand Delta; while, as heretofore stated, the most easterly one extends from the neighborhood of Cairo along the western shore of the Tennessee river, down the valley of the Warrior toward the coast of Alabama. The middle and main pebble stream evidently follows in general the course of the Mississippi River; but leaving it at the point where that river suffers its remarkable deflection eastward, we find the remnants of its ancient "bar" in the chain of the "Five Islands," which lie directly across the shortest line by which the Mississippi could reach the Gulf, and no doubt have had their share in causing this deflection.

Both the size of the pebbles carried by this middle bayou, and their character proving transportation from high northern latitudes, show it to have been the main channel during the Orange Sand epoch. It is not surprising, therefore, that in the direction of its course the Orange Sand formation should extend farther south than anywhere else. The pebble-beds are now overlaid by fine sandy materials, proving a diminished velocity, owing, doubtless, to a general depression, but greater at the north than at the south.

While the lateral bayous descending through Louisiana and Alabama were closed at the end of the Orange Sand epoch, it is evident that the central channel continued open; inasmuch as the next succeeding deposit, viz.: the Loess lies in a trough-shaped depression of the Orange Sand materials, the line of contact being always conformable and devoid of any trace of atmospheric denudation. The perfect peroxidation of the materials of the Orange Sand would seem, nevertheless, to point to a certain period of exposure to atmospheric agencies,

* Am. Jour. Sci., May, 1866; Miss. Rep., 1860, p. 26 and following.

caused by a temporary diminution of the influx of northern waters, through the cessation of subsidence, perhaps.

During this epoch of quiet might have begun the formation of those extensive swamp and lagoon deposits, the lower members of the Port Hudson series, whose floor stratum with its superimposed generations of cypress stumps, indicate a slow secular subsidence. The velocity of the latter seems gradually to have increased until the growth of old trees became impossible, and finally, in stratum No. 3 of the Port Hudson profile, we again meet the evidences of currents moving sand, pebbles, and drift-wood.

Then follows the Loess proper, a deposit utterly devoid, in Mississippi and Louisiana, of any evidences of fluviatile action —a uniform silt even in profiles of eighty feet, with scarcely a vestige of stratification, and none but terrestrial fossils.

The precise circumstances under which such a deposit could be formed, are perhaps a little obscure. There must have been such a depression of the whole country as to transform the immediate valley of the Mississippi, as far as Keokuk, as well as the valleys of the larger tributaries, into estuaries of the Gulf of Mexico, containing a mass of water too great to be sensibly affected by the variations now causing the annual overflows of those rivers (for otherwise the deposit must have shown lines of deposition), yet possessing a gentle flow above (since the materials of the bluff formation of Missouri and Indiana exhibit signs of fluviatile action); quite fresh in its upper portions (where fluviatile shells are found), but rendered unfit for the life of either a fresh or salt-water fauna by an admixture of sea-water, in its lower and almost stagnant portion, at tide level; and deriving its vestiges of animal life only from the "offscourings" of the adjoining unsubmerged lands.

Sir Charles Lyell* inclines to consider the Loess as the product of "successive inundations of a great river," the absence of stratification from such deposits having, apparently, an analogue in the alluvial deposit of the Nile. But the case is far from being analogous; for the same phenomena are still observed in the modern deposits of the Nile, and are clearly attributable to the peculiarities of the hydrographic basin of

*Principles of Geology, 10th edition, p. 464.

that river; whereas, in the modern alluvium of the Mississippi, it is exceedingly difficult to find a uniform stratum two feet in thickness. The Nile mud is each year derived from the same rivers of Abyssinia, and equalized by intermixture and subsidence during at least 1500 miles of its course. On the Mississippi, on the contrary, the deposits of different annual inundations are readily distinguished by the inhabitants for years afterward, according as the Illinois, the Missouri, Ohio, Arkansas or Red River happen to have furnished the main influx. The absence of any such differences from the Loess can only be explained on the assumption that the mass of water filling the channels was too great to be sensibly affected by such causes, the more so as the continental surface was sensibly diminished in consequence of a depression which, as far south as Fort Adams, cannot have been less than 400 feet, and on the coast not less than 200, but more probably the same as farther above.

The existence of the elevations on the Louisiana coast, above described, renders it necessary to assume that at the end of the period of depression — the "Champlain epoch" — the entire delta-plain (so called) west of the Mississippi was covered by the deposits of the Orange Sand and Port Hudson series to an equal height; and that during the succeeding "Terrace epoch" of elevation, the veritable Missi*ss*ippi — *our* Mississippi — swept away these deposits in excavating its present valley. At first it might sweep over or through the pebble ridge, but would finally turn to the direction of least resistance, leaving the "Five Islands" high and dry.

It would thus seem that, unlike other large rivers of the world which have from the outset added to the land by bringing down the materials to form their alluvial plain, the Mississippi has first formed by denudation the plain which it was subsequently to cover with its alluvial deposits to a comparatively inconsiderable depth. The western and southern limits of this denuding action would seem to be marked by the Grand Coteau des Opelousas, the Cote Gelée and the Five Islands; and the materials swept away from this area doubtless contributed largely to form the foundations of the truly alluvial plain extending south and south-eastward of lakes Maurepas, Pontchartrain and Borgne.

It is obvious how futile must be all attempts to estimate the age of the Mississippi river in absolute measure, by a comparison of the advance of its present delta into the Gulf, with the distance of its mouth from the divergence of bayous Plaquemine or Manchac. When the broad flood of the Terrace epoch contracted into the present Mississippi, that stream emptied into a sea rendered shallow by the deposition, within a comparatively short period, of a huge amount of material. Within such a sea its channel would be likely to change about, somewhat like those of the great rivers of China. *Now*, it is advancing into the deep water of the Gulf of Mexico, but at a very different rate, and by a very different process from that of simple alluvion. But the questions pertaining to this portion of the subject, together with the results of my observations in the Delta proper, I propose to discuss at a future time.

---

11. OBSERVATION ON THE RED QUARTZITE BOULDERS OF WESTERN IOWA, AND THEIR ORIGINAL LEDGES OF RED QUARTZITE IN IOWA, DAKOTA AND MINNESOTA. By C. A. White, M. D., of Iowa City.

PROFUSELY scattered among the drift material of western Iowa, are fragments and sometimes large masses of very hard red quartzite, so constant in its physical characters and appearance, and so different from any other variety of rock contained in the drift as to readily attract attention.

Judging from published descriptions of the rocks observed by various persons in eastern Dakota and south-western Minnesota, I ventured to suggest in an article published in the May number of the "American Journal of Science and Arts" for 1867, that the red quartzite boulders of the drift of western Iowa were derived from those rocks, while the associated granitic and other boulders had a still more northern origin. While continuing my observations to north-western Iowa during the present summer, it has been my good fortune to determine, in a great degree, the eastern limit of distribution of those quartzite boulders, and to confirm my opinion, then expressed, by tracing those boulders, step by step, to their

original homes in the ledges of rock which occupy the region before referred to, and to find the granitic boulders resting upon those ledges where they had been arrested in their icy journey from still more northern homes, and almost within the very grooves they themselves had made upon the quartzite.

The first exposure of the quartzite *in situ* was found in the extreme north-~~east~~ corner of Iowa, where its outcrop makes a slight fall in the Big Sioux River; the same exposure extending eastward about a quarter of a mile to the point where the surveyors have placed a post of the same rock to mark the south-west corner of Minnesota.

The rock is distinctly stratified and has a dip here of six or eight degrees to the northward. Northward from this point, particularly near the Big Sioux River, the same rock is frequently exposed. At Sioux Falls, about eight miles in a straight line from the last named locality, the rock has the finest exposure yet seen. Here by measurement and estimate of the dip, which is here four or five degrees to the *southward*, we found a thickness of about three hundred feet. Upon visiting the red pipestone quarry, thirty-five miles north-northeastward from Sioux Falls, we find the same rock enclosing the layer of red pipestone.

Dr. Hayden and others have shown that this rock occupies a large area in this region, but it seems that no one has heretofore proven that it was the original deposit of those red quartzite boulders so common in western Iowa and eastern Nebraska.

Very distinct glacial scratches were seen in many places upon the surface of this hard quartzite, the direction of which vary in different places from five to twenty-five degrees both east and west of south, indicating no doubt mere local shiftings of the glacial current.

At the red pipestone quarry a group of four or five immense granite boulders, known in Indian legends as the "Medicine Rocks," rest upon the surface of the quartzite which they and others have so smoothed that the Indians have long used the surface to trace hundreds of hieroglyphic figures upon, by pecking it with sharp pointed stones.

In another place the direction in which the glacier was moving is clearly indicated by the splintered condition of the flinty rock on the proximal or northern side of vertical cracks and fissures in the mass, while on the distal or southern side the angle formed by the vertical and upper surfaces was always found complete. This effect could not be produced with the same distinctness upon a softer or less flinty rock.

No opinion as to the geological age of these rocks is yet matured, but they are known to be older than the Inoceramus beds of Cretaceous age. After dilligent search in every locality visited, as well as in hundreds of the glacier-moved masses, I have failed to find any trace or resemblance of any fossil whatever.

It is worthy of remark, however, that it is metamorphism alone that makes the quartzite lithologically different from the sandstones and ferruginous conglomerates of the Dakota group as seen in Iowa and elsewhere, while chemically they are in no essential degree different.

Those sandstones contain an occasional thin bed of clay, such no doubt, as the pipestone originally was, and the same metamorphic action that converted the sandstone into a quartzite, doubtless also made the clay a pipestone.

---

12. On the Old Lake Beds of the Prairie Region. By Samuel Jacob Wallace, Keokuk, Iowa, 1868.

The prairie-region of central North America, is not a mere surface formed from sets of level continuous strata eroded into such forms as drainage will produce.

They are spread over series of great swells of elevation and intermediate or enclosed basins, with river systems marking their lowest lines of flexion, and worn outlets from the basins.

These general basins contain the coal field and other deposits formed before the wearing down of the river ways.

Besides the system of basins forming the declines from the general elevations, slopes and swells, on which the surface and common strata are spread, there is another type of depressions,

of more limited and definite boundaries, but less explicable origin.

These are usually referred to water-way erosions, without satisfactory explanation. Their forms are often extensions of greater or less length, from five to twelve miles wide, with remarkably direct, almost parallel and corresponding bluff side walls, lying at or near the low lines between great slopes of drainage. They are abrupt depressions of perhaps one to two hundred feet, by the discontinuance of solid strata and drift. Their floors are often flat lacustrine beds; in places, sand. Many have evidently formed lakes before the cutting down of the outlets, subsequent to the drift, and elevation of the surrounding country. Their form and position causes them to be occupied by general or local drainage ways; but without evident correspondence between their space and the general importance of the streams traversing them, which vary from almost nothing, up to the largest rivers.

Two principal instances of these depressions will be noticed. Both lie with their lines of extention near a north and south course. They form great curves to opposite sides. There is no apparent connection or system between them. The Mississippi River flows through one and partly through the other. Their north ends are both against east and west bluffs.

On the northern head bluff of one is the city of Muscatine, Iowa. After a length of fifty miles this depression abruptly abuts on another east and west bluff, at Dallas, Illinois.

Six or eight miles to the south-west from the south end of the first is a short depression six or eight miles across each way, in Iowa, west of Nauvoo, Illinois, connected with the first by a westward river channel; apparently worn down from an east and west trough of general curvature, like others southward in the vicinity.

Still beyond the last, to the south-west six or eight miles across broken high lands, is the north end of the second main depression. This has no connection with the first. It is in the State of Iowa, twelve miles north-west of the city of Keokuk. The Desmoines River enters it near this point, from a small channel, partly worn, between the hills from the north-west. The Mississippi River enters it at Keokuk through a channel

of evident erosion of a mile wide, from the north-east and north; but without noticeable impression upon it. Its course is south for fifty miles, then turning south-east, with a width of five to eight miles, increasing to from eight to twelve, and a whole length of over a hundred miles.

The Illinois River flows through such depressions; and they exist at various places over the country, but, apparently, are not yet fully traced out by surveys. We find them occupied by creeks and by small rivers, as chance seems to determine.

The eroding power of drainage streams, as indicated by the larger rivers where they have clearly worn out channels, assisted often by rapids, shows no sort of relationship to these spaces where great beds of horizontal solid strata disappear over areas scarcely conformable to the idea of stream erosion. I can point out no proper cause for them, but I must rest in directing attention to the real nature of the facts.*

* I have, to-day, June 21st, 1869, with my friend Dr. Geo. M. Kellogg, examined the old river shingle of angular Keokuk limestone fragments, overlying the Drift formation on the gently sloping river face of the bluff in this city. It is at a height, I suppose, of from sixty to ninety feet above the present river level. And is on the bluff curve toward the second main lake bed above described. This probably shows a former depth of nearly so much to the lake. Dr. Kellogg has heretofore viewed this shingle as indicating an ancient fall at this place. But I see no indications that the fall was not below the lake, a hundred miles away.

TITLES OF COMMUNICATIONS.*

1. On the Genus of extinct Sea-Saurians, *Elasmosaurus*, ... EDWARD D. COPE.
2. On the Artistic Evidence of the Remote Colonization of the North-Western or American Continent by Maritime People of Distinct Nationalities before the Modern Era, . J. H. GIBBON.
3. Effect of Atmospheric Changes on the Eruptions of the great Geyser of Iceland, ... P. A. CHADBOURNE.
4. Notes on the Defects of Lightning Rods, ... JAMES BUSHEE.
5. On the Boulder Field in Cedar County, Iowa, ... RUSH EMERY.
6. Phases of Glacial Action in Maine at the Close of the Drift Period, . N. T. TRUE.
7. Some Experiments on the Influence of the Physical Condition upon the Personal Equation, in Transit Observations, ... W. A. ROGERS.
8. The Calculation of the Crystalline Form of the Anhydrous Carbonates, Nitrates, Sulphates, Perchlorates, Permanganates, and other Salts of the composition A B_3 C, or A B_4 C, ... GUSTAVUS HINRICHS.

*The papers under this head were also read; of most no copy has been received for publication; of others it was voted that the title only should be printed. No notice, *even by title*, is taken of articles not approved.

9. Supplementary Notes on Gold-Genesis, HENRY WURTZ.
10. Upon the Ammonoosac Gold Field in New Hampshire, ... HENRY WURTZ.
11. On a Suspected Unknown Element in the Laurentian Magnetites, . . HENRY WURTZ.
12. On the Nature of Metallicity, . . HENRY WURTZ.
13. Studies of the Red Sandstones of the Atlantic Slopes, and their enclosed Igneous Masses, ... HENRY WURTZ.
14. Note upon the *Palæotrochis*, . . HENRY WURTZ.
15. Studies in Chemical Geogony. In three Parts:
    - I. On the Prozoic Atmosphere of the Zoic Dawn,
    - II. Zoic History, from a Chemical View-point,
    - III. Chemical Revelations of the Final Zoic Catastrophe, . HENRY WURTZ.
16. Bibliography of Entomology in the United States and Canada, since 1862, ... JOHN G. MORRIS.
17. On Some Points in the Geology of Vermont, ... T. S. HUNT.
18. On the Upper Silurian and Devonian Rocks of Ontario, ... T. S. HUNT.
19. On Gold in the Laurentian Rocks of Canada, ... T. S. HUNT.
20. On the Gold Regions of Nova Scotia, T. S. HUNT.
21. On the Chemico-Geological Relations of the Metals, ... T. S. HUNT.
22. The Antiquity of Man in North America, ... J. W. FOSTER.
23. Origin of the Prairies, . . . J. W. FOSTER.

24. Exhibition of the Crania of Boötherium and Castoroides, with Remarks on their Geological Position and their Living Analogues, . . J. W. FOSTER.

25. On the Occurrence of Fluor Spar in Southern Illinois, ... J. W. FOSTER.

26. On the Refrigeration of Continents, . J. W. FOSTER.

27. On the Occurrence of Tin in Missouri, J. W. FOSTER.

28. Migrations of the Indian Family, . L. H. MORGAN.

29. Hough's Barometrograph as applied to the Investigation of the Storm Curve, J. H. COFFIN.

30. Brief Remarks on the Botany, Meteorology, and Geology of Mount Mansfield, Vermont, ... JAMES HYATT.

31. Habits and Peculiarities of Plants in South Eastern New York and Vicinity, ... JAMES HYATT.

32. Movements in Stratified Rocks, since the Glacial Epoch, ... JAMES HYATT.

33. On the Stratigraphical Relations of the Fossil Horse in the United States, CHAS. WHITTLESEY.

34. On the Formation of Shells and Belemnites, and Phosphates of Iron at Mulica Hill, Gloucester Co., N. J., A. B. ENGSTROM.

35. Notice of some New Vertebrate Remains from the Tertiary of New Jersey, ... O. C. MARSH.

36. Further Notice of Experiments on Snow and Ice at a Temperature below 32° F. . . . . EDW. HUNGERFORD.

37. Experiment Illustrating the Flow of Glaciers, ... EDW. HUNGERFORD.

38. The Recent Contributions of Science to the Arts of Dyeing and Printing Woolen Tissues, ... JOHN L. HAYES.

39. The Hermaphrodism of Fungi ascertained, ... T. C. HILGARD.
40. The Confervaceæ ascertained to be Thelline; their Phase, Development, Circuit and Generation, . . . T. C. HILGARD.
41. Spermatic Phenomena. The Paludal Endogens a Class Intermediate between Endogens and Exogens, . T. C. HILGARD.
42. On the Mathematical Investigations made for the Construction of the Illinois and St. Louis Bridge, . WM. CHAUVENET.
43. On a Method of Measuring very Small Rectilinear Motions, . . . WM. CHAUVENET.
44. Relations of the Metamorphoses of the Phosphates to Waste and Repair, . E. N. HORSFORD.
45. Economy in the Conversion of Beef into Food, ... E. N. HORSFORD.
46. Remarks on the Galvanic Battery, . G. W. HOUGH.
47. On a proposed new Mechanism for the study of Galvanic Batteries, . . G. W. HOUGH.
48. Remarks on the total Disturbance of the Barometrical Column, . . G. W. HOUGH.
49. On the Geological Age and Equivalents of the Marshall Group, . A. WINCHELL.
50. Exhibition of a New Geological Chart, A. WINCHELL.
51. Exhibition of a New Label Holder for Zoölogical Specimens, . . . A. WINCHELL.
52. On some points in Geological Nomenclature, ... A. WINCHELL.
53. The Profiles of Blast Furnaces, . . THOS. EGLESTON.
54. Source of Muscular Power, . . E. N. HORSFORD.
55. Fossil Fishes, Insects, Crustacea, etc., of the Coal Measures of Grundy County, Illinois, ... A. H. WORTHEN.

56. Fuel Resources of Illinois, . . A. H. WORTHEN.

57. On the Physical Geography of the Continent of North America during the different Geological Periods, . . J. S. NEWBERRY.

58. On the Transportation of the Materials forming the Carboniferous Conglomerates, ... J. S. NEWBERRY.

59. On the Surface Geology of the Basin of the Great Lakes and the Upper Mississippi Valley, . . . J. S. NEWBERRY.

60. On the Plasticity of Rocks, and Origin of the Structure of the so called Gravestone Slates of California, . W. P. BLAKE.

61. On the Gradual Desiccation of the Western Portions of North America, W. P. BLAKE.

62. Vestiges of Prehistoric places in California, ... W. P. BLAKE.

63. Geological Section of Ohio, . . E. B. ANDREWS.

64. The Hot Term of July, . . . O. N. STODDARD.

65. The Higher Law of Correlation, . O. N. STODDARD

66. On Certain Physical Features of the Mississippi River, ... G. K. WARREN.

67. The Vertebral Type of the Cranium a Quinary one, ... T. C. HILGARD.

68. Extremities of the Skeleton Typically 5, T. C. HILGARD.

69. The Quebec Group in Northern New Hampshire, ... C. H. HITCHCOCK.

70. The Supposed Triassic Foot-marks in Kansas, ... C. H. HITCHCOCK.

71. The Distortions of Pebbles in Conglomerate at Rangely, Maine, . G. L. VOSE.

72. On *Elasmognathus* and its Relations to the Tapiridæ generally, . . . THEODORE GILL.

73. On the Classification and Relations of Seals, ... THEODORE GILL.
74. On the Secular Recurrence of Identical Petrogenetic Conditions, . . A. WINCHELL.
75. Superficial Geology of the Lake Shore near Chicago, ... J. S. JEWELL.
76. The Darwinian Theory of Development, ... CHARLES MORAN.
77. The Periodic Law in the Failure of Harvests and Inundations, with Suggestions as to their Insurance, . GEO. A. LEAKIN.
78. On the Progress and Present Condition of the Geological Survey of California, ... J. D. WHITNEY.
79. The Fossil Human Skull of Calaveras County, California, . . . J. D. WHITNEY.
80. Some Points in the Surface Geology of the Western Side of the American Continent, ... J. D. WHITNEY.
81. The Yosemite Valley, . . . J. D. WHITNEY.
82. On the Structure and Aqueous Origin of Gold-bearing Mineral Veins, . BENJ. SILLIMAN.
83. On Methods of Amalgamation in the Treatment of Gold Ores, . . BENJ. SILLIMAN.
84. On the Occurrence of the Mastodon in the Deep-Lying Gold Placers of California, ... BENJ. SILLIMAN.
85. Geodes, ... W. W. WILLIAMS.
86. Modern Discoveries in Palestine, W. W. WILLIAMS.
87. On the Molecular Arrangement of the Inorganic Acids, ... GEO. F. BARKER.
88. Theory of the Prediction of Star Places, T. H. SAFFORD.
89. Sketch of the Topography, Geology and Antiquities of the Caucasus, F. VON KOSCHKULL.

90. On Two New Fossil Trees, the oldest known, found by Rev. H. HERZER, in the Devonian Rocks of Ohio, . J. S. NEWBERRY.

91. Tides in Lakes, ... H. A. NEWTON.

92. On the Evaporative Power of the Sun near the Base of the Sierra Nevada, in Calaveras County, California, . BENJ. SILLIMAN.

93. On the Tides of Lake Michigan, . W. FERREL.

94. On Redemption Periods of Monetary Values which involve Life Contingencies, ... E. B. ELLIOTT.

95. On the Metrical Unification of International Coinage, ... E. B. ELLIOTT.

96. A Few Facts concerning the Vital Statistics of Amherst College, . . EDW. HITCHCOCK.

97. On the Archæological Value of Certain Ancient Beads, ... L. G. OLMSTEAD.

98. Exhibition of a New Selenograph, . JEROME ALLEN.

99. On a New Method of Measurement Map Drawing for Schools, . . JEROME ALLEN.

100. Glaciers as Extensive and Constant Geologic Agencies, . . . JAMES HYATT.

101. Points in the Geology of Hudson River, JAMES HYATT.

102. On Some of the Causes which Affect the Rapidity of Erosion of Rocks and of River Valleys, . . . WM. P. BLAKE.

103. On the Interpretation of Fossils, . B. W. HAWKINS.

104. Notes on the Origin of Bitumens, together with Experiments upon the Formation of Asphaltum, . . S. F. PECKHAM.

105. Note on Epitrochoidal Teeth, . . WM. WATSON.

106. On the Preservation of Meats, . . JOHN GAMGEE.

EXECUTIVE PROCEEDINGS

OF

THE CHICAGO MEETING, 1868.

HISTORY OF THE MEETING.

THE Seventeenth Meeting of the American Association for the Advancement of Science was held at Chicago, Illinois, commencing on Wednesday, August 5th, and continuing to Wednesday Noon, August 12th.

Two hundred and fifty-nine names are registered in the book by members who attended this meeting. Four hundred and seventy-five new members were chosen, of whom two hundred and sixty-one have signified their acceptance by paying the annual assessment, and, when practicable, signing the Constitution.

One hundred and fifty-one papers were presented, most of which were read, and some of them discussed at great length.

The sessions of the Association were held partly in the Library Hall of the Young Men's Association and other rooms of the same building, and partly in the Lecture Rooms of the First Baptist Church.

At about 10 o'clock A.M. on Wednesday the members were called to order by Ex-President NEWBERRY, who spoke as follows:

Gentlemen of the Association: The time has arrived to which we adjourned at our meeting last year, and it is my duty to call you to order for the organization of the Seventeenth Meeting of the Association. As the last, and perhaps the most agreeable part of my official duty, I take pleasure in introducing to you the President elect and my successor, Professor B. A. GOULD.

Dr. GOULD then said:

Gentlemen of the Association: It has been for many years the custom of our Association, before entering upon its proceedings, to ask the Divine blessing, and I will call upon the Rev. Dr. R. W. PATTERSON, of this city, to offer prayer.

Prayer was then offered by Dr. PATTERSON.

The Hon. J. Y. SCAMMON then delivered an address of welcome as follows:

MR. PRESIDENT AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:

The pleasant duty of addressing words of welcome to you has been assigned to me. They would come more appropriately from Mr. George C. Walker, the devoted President of the Chicago Academy of Sciences, to whose exertions the city of Chicago is greatly indebted for the present condition of that institution. But as he declined to relieve me of this duty, on his behalf and that of the Chicago Academy of Sciences, the Local Committee and the citizens of the Garden of the Lakes, I bid you welcome; and while we extend to you the hospitalities of the city and its citizens, we thank you for appointing this meeting of the Association in Chicago. We are a republican people, with no boasts of ancestry or hereditary distinction. Our community is composite, representing the nationality of every country in Europe, with a slight trace of Asia, Africa and the Isles of the ocean. In this fact we find occasion for the greatest hopes for the future, for so far as a nation or people represents universal humanity is its own capacity for perfection. The English language is stronger and richer than any other, because it is indebted to many different sources for its form and character, and the power of Great Britain is doubtless greatly owing to the fact that its population is more fully a representative of the various nations of the world than any other country in Europe. If this be so we may hope that the city of Chicago, and the country of which it is the centre, will, in time, present one of the fullest developments of humanity. In this city, the land of which was only surveyed into lots in 1830, and which now has a population of over a quarter of a million, there are more languages spoken, perhaps, than in any other American city, and Northern and Western Europe are most fully represented in its periodical press; and it is fit and becoming that your Association, that has for its objects "the advancement of science by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of North America, to give a stronger and more general impulse, and a more systematic direction to scientific research in our country and to procure for the labors of scientific men increased facilities and a wider usefulness," should hold its meetings in our midst. Our scientific and literary institutions have not much to present beyond the foundation stones, but as *Il n'y a que le premier pas qui coute*, we rejoice that we can show you a beginning not unworthy of a city of thirty years. We make no boast of our position or prowess, though we cannot fail to acknowledge in thankfulness and gratitude the abundance which crowds upon us and the facilities which are at our doors. We are surrounded by the granaries of the

world, and if the cattle of the thousand hills are not ours, the herds about us are more than the hills could hold. We look out upon a great interior ocean, upon whose surface float the white sheets of commerce, and we are connected by canals and iron bands in all directions. Our growth has been rapid, yet substantial, and probably no other city ever attained equal importance from voluntary causes in the same space of time. We are an outgrowth and development of the progress and science of the nineteenth century, and we desire, in welcoming you, to recognize the great movement of the last hundred years, which, passing in mighty power from the Old World to the New, penetrates from the coast to the interior, from the surface to the substance, from the apparent to the real, which elevates science from dead matter to the activity of real life, and raises man from a beast of burden to be the director and controller of all things below him, enduring, as it were, physical force with the skill and activity of human hands. Again, Mr. President and gentlemen, I bid you welcome, and assure you that all we can do to make your session among us agreeable and useful shall not be wanting. May your deliberations be harmonious, and the result of far-reaching importance, and may your assembling in this city again not be long delayed, and when we again meet you here, the course of empire, in its rapid march, may have travelled so far that we can no longer call ourselves a Western city. We may still find pleasure in repeating the lines of Bishop Berkley on the prospects of planting arts and learning in America.

> "Westward the course of empire takes its way.
> The four first acts already past.
> A fifth shall close the drama with the day.
> Time's noblest offspring is the last."

This address of Mr. Scammon was acknowledged by President GOULD, in behalf of the Members of the Association, as follows:

MR. SCAMMON: In behalf of the Association which I have the honor to represent on this occasion, let me express our hearty thanks for the words of welcome we have heard, and for the deeds which, forerunning your words, have given token of their sincerity. It is a source of delight to us that we are assembled in your noble city; and this delight has been enhanced a hundred fold by the cordiality and hospitality with which we have been welcomed. For we all know that this hearty welcome is not due to our possession of those gifts or honors which the world prizes. We are indebted for it to no repute of wealth, or power, or influence. We are hard-working men of science, whose lives, thoughts and energies are given to pursuits which the world deems shadowy and fruitless, and which are certainly unremunerative in the world's goods. We are not aspirants for the honors of the forum, or the tribune, or the market-place. Most of us may not even look to that well-deserved recognition which awaits the

successful teacher. But we are fervently and hopefully seeking to extend the bounds of human knowledge, and to contribute, each of us, something at least towards the progress of our race.

We cannot but prize that welcome most, which is offered us because of our aims and struggles in behalf of the cause which we hold dearest. And we find additional gratification because it comes from one whose name is associated with deeds of munificence in this city, and whose liberality has established its observatory, equipped with the largest refracting telescope in use in any public observatory of the world.

Once more, sir, accept our grateful thanks for the many acts of thoughtful kindness shown us by the Local Committee and the citizens of Chicago, and for your cordial words.

Then, addressing the Association, the President said:

GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: The rapidly revolving year has again brought us to the season of our annual convocation; and through your gratifying confidence and kindness it becomes my pleasant office to express, for all and to you all, mutual congratulations on the recurrence of this our time of scientific reinvigoration, and on the happy auspices under which we are gathered together.

In this magnificent city — sprung from the prairie shore like a creation of Aladdin's lamp, and welcoming us with a hospitality and heartiness which could not be surpassed — we may already see omens that the course of science, like that of empire just alluded to, "Westward takes its way." We have convened to-day at a point more westerly than any at which a scientific convention was ever before assembled. Yet here we find an active and earnest scientific spirit, a scientific academy full of vigorous life, doing its part toward the increase of human knowledge and toward disseminating the spirit of scientific inquiry. And in the cordially affectionate welcome given us, both collectively and individually, we rejoice to recognize tokens of regard for those pursuits to which we devote ourselves, and of which we enjoy the privilege of being regarded, for the time, as representatives.

Just twenty years have elapsed since the founders of this Association met together at Philadelphia; a little band, but full of hope and faith. In these two decades the Association has passed through all the stages of feeble infancy, of overweening confidence, of domestic dissension, of malady well-nigh fatal, of distressful bereavement, and of restoration to strength and usefulness. With our present meeting the years of nonage are at an end, and an era of renewed and increased opportunities for good dawns before us.

When, two years ago, we assembled after a period of suspended animation, at the beautiful and thriving city on the shore of Lake Erie, there seemed room for serious doubt whether the calamities which

had so seriously tried and imperilled the nation had not destroyed the very life of this Association. Thanks to the large-souled and warm-hearted citizens of Buffalo, our meeting there, although informally called and by no means largely attended, was eminently successful in reviving the old feeling of attachment to the Association; for the recollections of that meeting will always remain associated in our memories with unalloyed pleasure, and it insured for us in the ensuing year a meeting, the success of which has placed the vitality of the Association beyond all peradventure. And the spirit of cordial coöperation, of mutual regard, appreciation and encouragement, of kindly and courteous, yet honest criticism, which formed so marked a characteristic of these two meetings, affords grounds for confident anticipations of future success in that great work for which this organization was established — the Advancement of Science in America.

But, amid all the hopeful auguries and pleasant impressions which surround us here, the dark shadow, rarely absent from human festivals, has not failed to throw its gloom. There are some well remembered and honored countenances, familiar to our eyes a year ago, which we may never more see at our annual gatherings. And we would not fail in some tribute of respect and honor to three earnest workers in science who have, during the past year, been summoned from earth. The venerable botanist of Rochester, who for so long a period was never absent from our meetings, — the laborious and faithful astronomer of the Washington Observatory, who added to the lyre of heaven so many strings, — the accomplished engineer of Schenectady, once the General Secretary of the Association, — will long retain a place in your memories, and in the annals of the sciences to which they faithfully devoted themselves. And it would ill beseem us did we fail to recall with reverence the names of Chester Dewey, Ferguson, and William Mitchell Gillespie.

We now commence the Seventeenth Meeting of the American Association for the Advancement of Science. Let us not fail, through all its stages, to remember that its objects are best promoted by the union of kindly sympathy with ingenuous criticism. We seek the sharp encounter of mind with mind, yet not that any energy or power may be curtailed or rendered less effective, but rather that, as in the encounter of flint with steel, light may be evoked, where obscurity prevailed before.

Through the personal intercourse which these meetings promote between students in kindred departments of science, our meetings are undoubtedly even more serviceable to the great end in view than through the information contained in the formal communications. The experience of many years in our own land, as well as in Germany, England and France, shows that by far the greatest good is done outside the walls of the section-rooms. Ideas exchanged, suggestions offered, asperities softened, coöperations established, are the legitimate and constant fruits of these meetings.

Yet it is not only from the reciprocal exchange of ideas between individuals, and the public presentation of papers to the meetings, that the benefits of the Association flow. One of its chief functions is, by the migratory character of its meetings, to foster and diffuse a love of science and a taste for the investigation of nature. It is not among our functions to disseminate knowledge, but rather to attain it and to procure its attainment, and to conquer new truths from the domain of the unknown.

May I be pardoned if I presume to suggest yet another method by which the objects of the Association may be promoted, although it is one which for many reasons we have never yet essayed, and which until a very recent date it would have been both undesirable and futile to attempt. I refer to the institution of special researches in science by means of the funds of the Association, when these, by careful nursing and judicious management, shall permit of such an application.

Fifteen years ago, at the Cleveland meeting, this subject was discussed, and the opinion was found to be unanimous, that any attempt of the kind would then be injudicious. The progress of years has now changed the whole aspect and bearings of the case, and if a few hundred dollars should now be annually set aside as a fund to be placed in the hands of trustees, and the income of this ultimately be devoted to the prosecution of researches which would probably not otherwise be undertaken, a good work might be accomplished for the Advancement of Science. The great debt which many branches of science owe to the British Association for work of various kinds thus performed I need not recall to your minds. What might we not thus accomplish for the new science of meteoric astronomy, in the creation of which we may claim so large a share for our own countryman, Professor Newton? What might we not thus do for terrestrial magnetism? The expense of equipping and maintaining for five years a merchant vessel, provided with a few competent observers and instruments, would determine the magnetic constants for every point of the whole earth, and probably result in the development of facts and laws by which all the physics of the globe, and all geology, would be essentially advanced, — manifold more indeed than by the continuance in activity of every magnetic observatory now existing. Ancient astronomical observations might be reduced, and a contribution to stellar astronomy thus obtained, of incomparably greater value than would be afforded by observations made to-day. Tables of physical constants might be prepared, expeditions of exploration might be equipped, or costly yet important experiments carried out. That our Association will ultimately accomplish such work as this is among my most earnest convictions, as it is among my fondest hopes.

But I will not detain you longer. Let us pass to our work, bringing each of us what little we have been able to attain for the increase of human knowledge, in the hope that our several contributions may,

when added to those of the past and future, take shape and form, becoming centres of crystallization and growth for truths yet unattained. Let us strive to promote our own progress and that of others, in the great work to which we have dedicated our lives and such fortunes as we may possess, by mutually inciting each other to new effort, and by invoking that mental condition which science ought to foster,—humility before the vast unknown, mingled with that confidence which should animate the explorer.

Gentlemen, let me offer my sincere thanks for the great honor which your partiality has conferred upon me, and of which it shall be my aim to render myself more worthy hereafter. In the fulfilment of the duties of my office I shall look to you for aid and counsel, and shall rely upon your kindness to palliate my errors, and to attribute them, as you certainly may do, to no motives inconsistent with an earnest desire for impartiality, and for the best welfare of the Association.

The Association then proceeded to the election, by ballot, of six additional members of the Standing Committee, according to the requirement of Rule 4 of the Constitution. The names of those chosen are printed elsewhere with the names of the other members of that Committee.

The Association voted to hold its next meeting at Salem, Massachusetts, beginning on Wednesday, August 18, 1869. The officers elected for the next meeting are:

Colonel J. W. FOSTER, of Chicago, *President;* Professor O. N. ROOD, of New York, *Vice-President;* Professor O. C. MARSH, of New Haven, *General Secretary;* Dr. A. L. ELWYN, of Philadelphia, *Treasurer.* Professor JOSEPH LOVERING was elected, at Burlington, *Permanent Secretary* for another term of two years, commencing with the Chicago Meeting.

On Wednesday evening a general meeting was held in Library Hall, when President F. A. P. BARNARD delivered his address as retiring President, which had been postponed from the last year; and, on Thursday evening, a general meeting was held in the same place, when the President, B. A. GOULD, delivered an eulogy upon the late A. D. Bache.

A communication was received from Professor BENJAMIN PEIRCE, Superintendent of the United States Coast Survey, in relation to the changes now occurring in the star *Eta Argus* and the surrounding nebula, and recommending to the Association to express its opinion on the extreme importance of observations upon this nebula to be made in South America. The subject was referred to a committee, which will be found in another part of this volume.

Professor F. A. P. BARNARD proposed the appointment of a committee to express the congratulations of this Association to Dr. Ehrenberg, of Berlin, on his approaching Jubilæum, and enforced

his recommendation with the following remarks: "It has long been the custom in Europe, whenever any eminent contributor to science approaches the half century of his doctorate, to send to him addresses and testimonials, expressive of the respect and honor in which he is held. This is often done by individuals, but all organized associations are accustomed to send their resolutions. The American Academy of Arts and Sciences, in Boston, has already designated two of its members, Dr. Asa Gray and Professor Joseph Lovering, to present *in person* the congratulations of that Academy. In this case the tribute is peculiarly merited. Few men have done so much for the department of science in which the proposed recipient of honor has been engaged. He found the use of the microscope, in the study of natural history, almost barren. He stimulated it to its present great and high utility, opening such a world of investigation that even the instrument itself was perfected in the contemplation of it. The result has been the disclosure of a universe to be studied as multiform as that which the telescope reveals. No man ever presented to the world so much new matter as Ehrenberg in his book on *Infusoria*. He is now seventy-four years of age, and to his great labors he has sacrificed his eyes, and, by a recent accident, he has lost the power of locomotion. It appears, in view of these facts that America should join hands with Europe in paying a tribute of respect and gratitude to the man who by his great labor has procured such great results."

Many of the members in attendance accepted the private hospitality generously offered by families in the city of Chicago.

On Saturday, August 8, after the session of the morning, which was continued in the evening, the members of the Association and their ladies, accompanied by many ladies and gentlemen of Chicago, by invitation of the Local Committee, embarked on board the Steamer *Orion* and enjoyed an excursion upon Lake Michigan, visiting the Crib, and partaking of a bountiful lunch on board the Steamer.

During the meeting of the Association in Chicago, the members and their friends were elegantly entertained, on successive evenings, by Messrs. John B. Drake of the Tremont House, Wm. E. Doggett, N. S. Davis, and J. Y. Scammon, at their private residences, and Gage Brothers and Walters of the Sherman House.

Through the extraordinary liberality of the Directors of many of the railroads which centre in Chicago, the members of the Association had an opportunity after the final adjournment of the meeting, to make excursions to points of scientific interest, and not a few availed themselves of the privilege to visit the Coal Valley near Rock Island, the Mines of Lake Superior, or the newly planted germs of a city on the Missouri River, OMAHA. A small party proceeded from Omaha, by the invitation of Hon. W. B. Ogden, and Capt. J. B. Turner, over the whole length of the Union Pacific Railroad to a point more than

sixty miles beyond Benton, and, on the afternoon of August 17, after dining in the construction-train of General Casement, rode over a mile of road which had been finished with the rails which their own train had brought forward two hours before.

RESOLUTIONS ADOPTED.

ON THE CINCINNATI OBSERVATORY.

Resolved, That this Association cordially sympathizes with the effort now making by the Astronomical Society and the citizens of Cincinnati, to resuscitate the Cincinnati Observatory, and earnestly desires its success.

ON THE JUBILÆUM OF DR. EHRENBERG.

Whereas this Association is apprized that the veteran naturalist, CHRISTIAN GOTTFRIED EHRENBERG, will complete during the present year the fiftieth year of his Doctorate in Medicine, and whereas the Association sincerely desires to express their high appreciation of the eminent services rendered to Natural History, and especially to the knowledge of microscopic organisms, by this distinguished and laborious investigator and observer; therefore

Resolved, That a committee of three* be appointed to prepare an address to be presented to Dr. Ehrenberg, on the occasion of his approaching Jubilæum, congratulating him on the long and honorable career which it has been permitted him to enjoy, and assuring him of the deep veneration and respect in which his name is held in America.

Resolved, That the address, when prepared, be forwarded to the Hon. George Bancroft, Envoy Extraordinary and Minister Plenipotentiary of the United States of America to the Confederation of Northern Germany, with the request that he present the same to Dr. Ehrenberg, in the name of this Association, on the occasion of his Jubilæum, on the 5th of November next.

ON AN INTERNATIONAL COPYRIGHT.

Resolved, That it is the conviction of this Association that no product of human industry is, in a stricter sense, the rightful property of the producer than that which embodies in written form the results of intellectual labor.

Resolved, That literary property, in whatever country produced, ought, equally with every other description of property, to be under the protection of law, and to enjoy such protection equally in all countries.

Resolved, That this Association cordially approves and endorses the efforts which are making by the *Copyright Association*, recently organized for the United States of America, to secure the recognition by all governments of the rights of authors to their literary productions, and sincerely desires that these efforts may meet with early and complete success.

ON THE ESTABLISHMENT OF AN OBSERVATORY ON THE LINE OF THE UNION PACIFIC RAILROAD.

Resolved, That this Association recommends to the attention of those who would make intelligent and munificent endowments of scientific institutions, the importance of an Astronomical Observatory at some point on the Pacific Railroad between Nebraska and the Pacific Coast, and at as high an altitude as possible, where the clearness of the atmosphere, and the great number of cloudless days, would ensure remarkable and unsurpassed opportunities for astronomical observations.

* Afterwards increased to seven.

VOTES OF THANKS.

Resolved, That we owe to the accomplished Chairman of the Local Committee and to its indefatigable Secretary, and to the assiduities of the various subcommittees, a very large share of the order, success, and the unusual personal enjoyments which have characterized this meeting.

Resolved, That the thanks of this Association be given to the Young Men's Library Association, to the Caledonian Club, and to the Trustees and Officers of the First Baptist Church in Chicago, for the ample accommodations which they have freely placed at our disposal.

Resolved, That the thanks of this Association be presented to the editors of this city for the full reports of its proceedings published in their columns.

Resolved, That the thanks of the Association be presented to the Academy of Sciences, the Historical Society, the Young Men's Library Association, the Board of Trade, and to all other bodies, scientific, literary or commercial, who have freely opened to our use their rooms and collections, for the many courtesies which have contributed so much to the success of the meeting and the enjoyment of the members.

Resolved, That we heartily appreciate the hospitality which has been generously extended to the members of the Association, individually and collectively, by the citizens of Chicago, and the opportunity enjoyed by us of social intercourse with the ladies and gentlemen of the place at the receptions, in hotels and private houses, which welcomed our arrival here, which have cheered our leisure moments from day to day, and which have continued even to the hour of our last farewell.

Resolved, That the members of this Association acknowledge their great indebtedness to the Directors of many of the Railroads, over which they have passed in coming from their homes to this distant place of meeting, for the liberal terms upon which they have been carried; and that they are placed under peculiar obligations to the Directors of the Railroads which diverge from Chicago, and especially of the Chicago and Northwestern Railroad, for their extraordinary liberality in furnishing free passes to Rock Island, Omaha, Lake Superior, and other points of great scientific interest.

Resolved, That the thanks of the Association be presented to the retiring President, for the dignity, ability and impartiality with which he has filled the chair during the present meeting.

Railroad and Steamboat Corporations which have charged only HALF FARE *to members of the Association in attendance on the Chicago meeting.*

Chicago and Northwestern,
Chicago and Rock Island,
Chicago, Burlington and Quincy,
Chicago and Alton,
Illinois Central,
Michigan Central,
Michigan Southern,
Chicago, Columbus, and Indiana Central,
Grand Trunk,
Great Western,
Louisville and Nashville,
Mobile and Ohio,
Mississippi Central,
New Orleans and Jackson,
South Carolina,
Orange and Alexandria,
Wilmington and Weldon,
New Jersey Central,
New Jersey,
Philadelphia and Erie,
Pennsylvania,
New York and Erie,
Connecticut River,
Eastern of Massachusetts,
Boston and Montreal,
Boston and Ogdensburg,
Boston and Maine.

REPORT OF THE PERMANENT SECRETARY.

THIS report includes the executive business which belongs to the interval between the commencement of the Burlington meeting (August 21, 1867) and that of the Chicago meeting (August 5, 1868).

The Association now numbers four hundred and fifteen members. Only fourteen individuals joined the Association at Burlington. One hundred and thirty-two old members were struck from the list, on account of three years' delinquencies in the payment of assessments, their aggregate indebtedness amounting to eleven hundred and eighty-eight dollars ($1188).

The financial condition of the Association is as follows:

Between August 21, 1867 (the first day of the Burlington meeting) and August 5, 1868 (the first day of the Chicago meeting), the income of the Association, *including the balance from last year's account*, was fourteen hundred and twenty-two dollars and seventeen cents ($1422.17), of which fifty-two dollars and fifty cents accrued from the sale of the printed Proceedings, and the remainder from the annual assessments.

The expenses of the Association during the same period were twelve hundred and sixty-two dollars and sixty-one cents, which may be apportioned thus:

Cost of paper, printing and binding the Burlington volume of Proceedings, five hundred and seven dollars and forty-seven cents, ...	$507.47
Charges connected with the Buffalo and Burlington meetings two hundred and nine dollars and ninety-two cents, . . .	209.92
Salary of the Permanent Secretary, five hundred dollars, . .	500.00
Postage, stationery, express, etc., ...	45.22
	$1262.61

The details may be found in the cash account of the Secretary, which is herewith submitted as a part of his report. The balance in the hands of the Permanent Secretary, August 5, 1868, is one hundred and fifty-nine dollars and fifty-six cents ($159.56). There is no balance in the hands of the Treasurer.

JOSEPH LOVERING,
Permanent Secretary.

Chicago, August 5, 1868.

CASH ACCOUNT OF THE AMERICAN ASSOCIATION in

Dr.

Bigelow's bill for programmes,	$3.00
Harris's bill as clerk,	16.37
Postage,	27.00
Ripley's bill for circulars,	12.00
Carter's bill for envelopes,	1.25
Smithsonian Institution for express,	3.40
J. H. Abbott, for binding,	2.00
Harris's bill for copying records,	13.25
Clerk for preparing circulars,	5.00
Clarke, for twenty-one reams of paper,	115.50
Kilburn, for wood-cuts,	6.00
Paper for cover,	6.48
Paper and envelopes,	3.75
Ripley, for printing,	8.50
Paper for copying records,	1.80
Abbott, for binding Burlington Proceedings,	29.99
Express charges,	9.82
Expenses of Newport and Buffalo Meetings,	150.00
Salary of Permanent Secretary,	500.00
Dakin's bill for printing Proceedings,	347.50
	$1262.61
Balance to next account,	159.56
	$1422.17

PERMANENT SECRETARY.

Account with JOSEPH LOVERING. Cr.

Balance from last account,	$422.90
Assessments (from No. 421 to No. 651 of Cash Book) including the sale of Proceedings,	999.27
	$1422.17

List of European Institutions to which Copies of Volume XVI of the Proceedings of the American Association were distributed by the Permanent Secretary in 1868.

Stockholm, — Kongliga Svenska Vetenskaps Akademien.
Copenhagen, — Kongel. danske Vidensk. Selskab.
Moscow, — Société Impériale des Naturalistes.
St. Petersburg, — Académie Impériale des Sciences.
" " Kais. Russ. Mineralogische Gesellschaft.
" " Observatoire Physique Centrale de Russie.
Pulkowa, — Observatoire Imperiale.
Amsterdam, — Académie Royale des Sciences.
" Genootschap Natura Artis Magistra.
" Zoölogical Garden.
Haarlem, — Hollandsche Maatschappij der Wettenschappen.
Leyden, — Musée d'Histoire Naturelle.
Altenburg, — Naturforschende Gesellschaft.
Berlin, — K. P. Akademie der Wissenschaften.
" Gesellschaft für Erdkunde.
Bonn, — Naturhist. Verein der Preussisch. Rheinlandes, &c.
Breslau, — K. L. C. Akademie der Naturforscher.
Dresden, — K. L. C. Deutsche Akademie der Naturforscher.
Franckfurt, — Senckenbergische Naturforschende Gesellschaft.
Freiburg, — Königlich-Sächsische Bergakademie.
Göttingen, — Königl. Gesellschaft der Wissenchaften.
Hamburg, — Naturwissenschaftlicher Verein.
Hannover, — Die Naturhistorische Gesellschaft.
Leipsic, — Königlich Sächsische Gesellschaft der Wissenschaften.
Munich, — K. B. Akademie der Wissenschaften.
Prag, — K. Böhm. Gesellschaft der Wissenschaften.
Stuttgart, — Verein für Vaterländische Naturkunde.
Vienna, — K. Akademie der Wissenschaften.
" K. K. Geographischen Gesellschaft.
" Geologischen Reichsanstalt.
Württemburg, — Der Verein für Vaterländische Naturkunde.
Basel, — Naturforschende Gesellschaft.
Bern, — Allgemeine Schweizerische Gesellschaft.
" Naturforschende Gesellschaft.
Geneve, — Société de Physique et d'Histoire Naturelle.
Neuchatel, — Société des Sciences Naturelles.
Bruxelles, — Académie Royale des Sciences, &c.
Cherbourg, — Société Académique.
Dijon, — Académie des Sciences, &c.
Liège, — Société Royale des Sciences.

Lille, — Société Nationale des Sciences, de l'Agriculture et des Arts.
Paris, — Institut de France.
" Société Philomatique.
" Société Météorologique de France.
Turin, — Accademia Reale delle Scienzie.
Madrid, — Real Academia de Ciencias.
Cambridge, — Cambridge Philosophical Society.
Dublin, — Royal Irish Academy.
Edinburgh, — Royal Society.
London, — Board of Admiralty.
" East India Company.
" Museum of Practical Geology.
" Royal Society.
" Royal Astronomical Society.
" Royal Geographical Society.
Manchester, — Literary and Philosophical Society.
Batavia, — Société des Arts et des Sciences.